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Art. XVI.—*Waterglass;* by J. M. Ordway.

Part I.—History and Manufacture.

The soluble alkaline silicates have of late years come into extensive use in the arts, and seem likely to constitute a large and permanent branch of chemical manufactures. The common works on technology do not treat of them as fully as their present importance demands, nor can all that could be desired be found within the limits of any single paper hitherto published on the subject.* It is here proposed therefore to collect details from various scattered memoirs, as well as from private experience, and present a concise but more complete account of the history, manufacture, nature, and uses of the soluble silicates of potash and soda.

The later alchemists were acquainted with the deliquescent tersilicate of potash,—$\dot{K}_3\tilde{S}i$,—the basis of *liquor silicum*. Thus Glauber, in giving his second method of testing sand for gold, says:—"Take one part of white quartz or sand, mix it with three or four parts of salt of tartar, or any other alkali, and put the mixture into a crucible, but so as not to fill more than one-third of it; since otherwise, in melting, the mixture would rise and flow out of the crucible. Let it stand [in the fire] half an hour that it may be well ignited and changed to a pellucid glass.

* A pretty full resumé of all that had been published on Waterglass up to 1857, is given by E. Kopp in the Moniteur Scientifique, tome i, livraison 4me.

Pour it out and dissolve in water, or better, in lye; and the sand or silex will be dissolved and changed into a thick water."*

In 1825 there appeared a memoir in German "On a New and Useful Product Obtained from Silex and Potash, by Dr. Johann Nepomuk von Fuchs," in which we find the following account of the first discovery of soluble glass, or, as Fuchs provisionally named it, *waterglass*:—"I obtained it first about seven years ago, in pouring concentrated potash lye on very finely divided silica which had been precipitated from *liquor silicum* with sal-ammoniac, and well dried. The potash was absorbed by the silica with a sensible elevation of temperature, and the whole soon changed into a very firm, transparent, glass-like mass which proved to be permanent in the air. It did not occur to me that the same thing might also be produced by dissolving silica in potash and evaporating the fluid; since I then, and for a long time afterwards, held with all chemists the erroneous notion that a combination of silica and potash, to be permanent in the air, must be insoluble in water, and that a soluble product must necessarily be deliquescent. Two years afterwards, when I wished one day, for analytical purposes,—for which I first brought silicate of potash into use,—to prepare some as fully saturated with silica as possible, I learned to procure the substance under consideration, by the method of solution. For this purpose I took freshly precipitated silica, poured on it as much potash lye as I judged to be necessary for its solution, and brought it to a boil. The silica very soon disappeared, and, in order to saturate fully the alkali present, to my no slight astonishment, I was obliged to add a quantity of silica still greater than I had taken at first. After this was done the solution was boiled a long time to concentrate it, and thus came to the consistency of syrup; and on the surface there appeared a tough pellicle which dried in the air to a transparent glass. All bodies which came in contact with this fluid received a glassy covering that attracted no moisture from the air but became much the harder and more brittle. From this I saw that the product before me was the same which had been obtained before by the process of absorption."

The author goes on to say that the burning down of the theatre at Munich contributed to the completion of his discovery. For when the new building was to be erected, diligent inquiry was made for something to protect wood against fire; and many substances having been tested and rejected, it occurred to him

* Novum Lumen Chymicum, Amstelodami, 1664. A somewhat earlier mention of the same substance is made by Van Helmont. J. F. Gmelin quotes the following passage from his 'De Lithiasi,' published in 1644:—"Porro lapides, gemmae, arenae, marmora, silices, &c., adjuncto alcali vitrificantur: sin autem plure alcali coquantur, resolvuntur in humido quidem: at resoluta, facili negotio acidorum spirituum, separantur ab alcali, pondere pristini pulveris lapidum."

to make trial of the waterglass. Therefore with Dr. Pettenkofer as an associate, he made many experiments which showed its entire efficiency as a protective agent, and it soon assumed a greater importance than he had at first attached to it. They then labored to find some cheap and easy mode of preparation, which might render waterglass available for economical uses. In the first place they made *liquor silicum*, and dissolved in one portion the well washed silica precipitated from another portion. This method was too costly, and the product was not all that could be desired, since it contained too much carbonate of potash. Finally by trying the direct fusion of various mixtures of potash, sand, and charcoal, they got a product which, when powdered, dissolved slowly but almost completely in boiling water, affording a solution free from carbonic acid and perfectly saturated with silica.

In making the article for actual use they took for each charge 30 lbs. of well purified potash, 45 lbs. of quartz sand, and 3 lbs. of powdered wood charcoal. This was melted in a refractory crucible, and at the end of five or six hours, when the well fused mass had subsided into a quiet state, they dipped it out with an iron ladle and threw in a fresh charge.

Fuchs found in one sample of the dissolved silicate proportions corresponding to $\dot{K}_2\ddot{S}i_5$. In another analysis he obtained somewhat more silica and less alkali.

They also made a silicate of soda with such relative quantities of materials as should give $\dot{N}a_2\ddot{S}i_3$; but in Fuch's last work, published since the author's death, he recommends 45 lbs. of quartz, 23 lbs. of dry carbonate of soda, and 3 lbs. of charcoal, which would make $\dot{N}a_2\ddot{S}i_5$. Perhaps the recommendation was given on theoretical grounds, for such a product is altogether too hard of solution for practical use. In fact Fuchs, for most of his experiments, took potash waterglass, and the potash silicates are more soluble than the corresponding soda compounds.

Since 1825, owing to the enormous extension of the soda manufacture, the potash and soda salts have exchanged places as far as commercial value is concerned; and now no one thinks of using the potash salts except for those cases in which they are peculiarly suitable. Silicate of soda therefore is the article more commonly employed at the present time, and as found in commerce it varies from $\dot{N}a\ddot{S}i_3$ to $\dot{N}a_2\ddot{S}i_4$.

For designating the substances under consideration the Anglicized German name 'waterglass' is very convenient as a generic term applicable to either the potash or the soda glass, to the solid or the liquid product. It suits the genius of our language and should be generally adopted in preference to the less concise and less accommodating expression 'soluble glass.' It is true, an unpleasant recurrence of sounds may be got rid of by calling

a 'solution of soluble glass' 'liquid glass,' but we thus introduce a second term which is no less objectionable because it can be applied to the article only in a single state. The word 'waterglass' is comprehensive enough to express every variety and form of the substance discovered by Fuchs, while it properly excludes silicated alkali or 'liquor silicum,' the "kieselfeuchtigkeit" and "kieselsaft" of the Germans. Yet the two things are confounded by many. Thus while writing this, I have before me the printed directions given by an English manufacturer for dissolving his "soluble glass to produce liquor silicis;" and of two specimens of his glass, one proves to be a sesquisilicate of soda, and the other a bisilicate.

Still though the extremes are unlike enough, there can be only an arbitrary line of distinction drawn on the middle ground. Anything more alkaline than $\dot{N}a_2\ddot{S}i_3$ is prone to deliquescence, and is too poor in silica for most uses, and it would perhaps be but just to consider 'waterglass' as including nothing more basic than the sesquisilicates. Again with regard to this last term it may be remarked that as silica in combining with the alkalies is regardless of precise atomic ratios, 'silicate' without a limiting prefix, conveys no more definite idea than 'hydrate;' and the world being still divided respecting the atomic constitution of silicic acid, a limiting prefix has to be itself interpreted, unless the context makes its meaning apparent. In this paper silica is taken as SiO_3, merely because custom has made this the more familiar formula.

The fact that silicate of soda is not, like alum or rock salt, an article of definite and invariable composition, is not generally appreciated by consumers, who often absurdly judge of the goodness of a sample by the greater or less time required for dissolving it, and by the hydrometer strength of a solution made from a given weight. A just estimation would take into account the quantity of insoluble matter, the amount of saline contamination, and the relative proportions of soda and combined silica. A dry silicate may have mixed with it no inconsiderable percentage of carbonate, sulphate, sulphid, and chlorid, which greatly impair its quality, though they help to raise the hydrometer. And then again the greater the proportion of alkali, the more quickly may the solution be effected. But for any use to which the article is applied at present, it ought not to contain more than two equivalents of soda or potash to three equivalents of silica, and therefore should properly be rather hard to dissolve.

Manufacture.—It was discovered some years ago that flint or quartz enters into solution when boiled with caustic alkalies under strong pressure; and this method has sometimes been resorted to for manufacturing the silicates of potash and soda.

Thus to obtain a liquid for making 'Ransome's artificial stone,' the direction is given to prepare, in the first place, a perfectly caustic lye with soda ash and lime, and subsequently add enough caustic baryta to decompose the sulphate of soda present. The clear liquor is then to be heated some thirty-six hours to about 300° F., in an iron boiler containing a wire cage filled with broken flints.* The solution is afterwards concentrated by evaporation in open pans. A sample of the substance thus produced is said to have contained 47 per cent of $\dot{N}a_2\bar{S}i$.† This would be a *liquor silicum*, and altogether too alkaline for most uses.

A very rational improvement of the process is to use infusorial silica or those minerals consisting of silica in its active modification, instead of inert quartz. Bergeat proposes also the fine residue left in the manufacture of sulphate of alumina from porcelain clay.‡

I have not met with any published statement showing the maximum amount of silica which may practically be brought into combination in this way. But even supposing that the alkali could be fully saturated, there are still strong objections to this high pressure system. In fact a more tedious and costly way could hardly be devised, unless we go back to Fuchs' original plan of dissolving precipitated silica in potash lye.

By operating in the dry way, the work may be better done, in less time, and with less expensive materials, less trouble, and less costly apparatus. The easiest method is to fuse sand with carbonate of potash or soda. The cheapest way yet devised,—and the one therefore which seems likely to supersede all others eventually,—is to decompose a sulphate with quartz sand and carbon. It has indeed been suggested that silicate of soda might be made directly from chlorid of sodium, with the aid of overheated steam. It is very doubtful, however, whether any apparatus could be got up, capable of resisting at the same time the very great heat required and the action of the vaporized chlorid of sodium, of the gases passing off, and of the intensely heated silicate remaining. Otherwise such a process might be carried out by passing the mixed vapors of water and salt through an excess of strongly ignited sand,§ provided the product were quite infusible at the temperature of decomposition. But when a substance fluxes, and especially when, like glass, it melts to a tough mass, or at least agglutinates, the action of vapors or gases on it can be only superficial and therefore must be exceedingly slow. In the roasting of ores, for instance, the metallurgist has

* A figure of the apparatus used is given in Ure's Dictionary of Arts, Manufactures, and Mines,—latest Am. Ed.—Art. 'Stone, Artificial.'

† Knapp's Technology,—Am. Ed.,—ii, 398.

‡ Wagner's Jahresberichte über die Fortschritte der Chemisches Technologie,—iv, 205.

§ See this Journal [2], vi, p. 260–266.

to exercise the greatest care to keep the matter under treatment in a porous condition, or else many of the metallic sulphids would volatilize sooner than oxydize. And there seems to be no good reason why the same principle should not hold true in oxydizing by steam as in oxydizing by air. The application of Tilghman's process to the manufacture of silicates from simple alkaline chlorids,—however beautiful the method may be in theory,—does not then *a priori* promise advantageous results. The product would at best be too siliceous, and to make it soluble by remelting with carbonate of soda, as proposed by E. Kopp, would add too much to the cost.

Waterglass has sometimes been prepared, as by Fuchs and Pettenkofer, in large crucibles or in the common pots of a glass furnace; and when but little is wanted or a very pure product is desired, this may be the better way. But as the consumption is now very large, and absolute purity is seldom required, most manufacturers use reverberatory furnaces. For fuel I have known anthracite to be burned, though bituminous coal is far better and more economical. Some make an intricate mixture of sand and soda ash by grinding or sifting them together, and heap it up on the bed of the furnace till there is only room enough between the charge and the arch for the products of combustion to pass along,—a little vacant space being reserved just within the charging door. The mixture continually fuses on the surface and runs down to this vacant space, and the fluxed product is drawn out, a little at a time, as it collects. When the whole charge is thus disposed of, a fresh one is thrown in. This method is indeed rude and unsatisfactory, and imposes inconvenient restrictions. Yet under the care of a judicious workman, when the materials are of the best quality and the fire burns well, the result is much better than might be expected. Commonly, however, the silicate so made is exceedingly variable, that of no two drawings being exactly alike. It varies in color from almost white, through every shade of brown, to black. It contains more or less unchanged sand and undecomposed carbonate of soda as well as sulphid and chlorid of sodium and sulphate of soda. In specimens of the article so prepared and intended to be alike, I have found the quantity of dry soda to vary from 24 to 33 per cent, while the uncombined sand sometimes amounted to ten per cent. In this mode of working the consumption of fuel is comparatively small, and the wear of the furnace is slight, but, on the other hand, the thorough mixing of the materials requires no little labor, and the product is rather fritted than completely vitrified.

A better plan is to throw into the furnace but a moderate quantity of the rudely mixed ingredients, keep the whole charge in till it is seen to be well done, and then draw it all at once.

Thus the mass is completely under the control of the workman, perfect decomposition may be insured, and the product can be made uniform in color and quality. In the strong heat, the chlorid of sodium is mostly volatilized, and much of the sulphate is decomposed and changed to silicate. But there usually remains enough sulphid of sodium to communicate a brown tinge. The hotter the furnace is, the easier is it to get a light colored article. The brown color may be completely removed by throwing in a few pounds of arseniate of soda,—or a mixture of arsenic, soda ash, and nitrate of soda,—and stirring it well in just before drawing the charge. Antimoniate or stannate of soda would answer as good a purpose and would be safer to handle. Still I have never known any injury to health to result from using the arseniate.

With a furnace whose bed had an area of 24 square feet, the grate being 3 ft. by 2 ft., and consuming about 83 lbs. of Pictou coal per hour, I have worked four charges in 24 hours, each consisting of 250 lbs. of soda ash,—80 per cent,—and 315 lbs. of pure quartzose sand. The well fused mass was decolorized with about 4 lbs. of arseniate of soda, and then drawn out into a kettle full of cold water kept constantly renewed. As foreign salts can only be mechanically mingled with the melted glass, any particles of sulphate of soda that have escaped decomposition, are dissolved and washed away as the fluid mass breaks up into minute fragments by contact with water. The glass so obtained had a very slight greenish color and was quite pure.

To make a more readily soluble sesquisilicate for calico printers, the charges consisted of 260 lbs. of soda ash and 250 lbs. of sand.

Fuchs directs an addition of coal dust to be made to the materials, but when the heat is strong, such an addition is entirely unnecessary. The drawing into cold water causes but little loss when we have bisilicate of soda to deal with, though with anything more alkaline than sesquisilicate of soda or bisilicate of potash, the waste is too considerable. When waterglass is to be dissolved at the manufactory, it may as well be drawn into water, for this saves the expense of grinding. But when it is to be sold in the solid form, water drawing cannot be recommended; since after the silicate has once been wet, it is almost impossible to get it dry again, and if it is packed in casks, the particles soon cohere into one solid, unmanageable lump. To avoid such a difficulty the melted mass may be received into a thick cast iron pan or half cylinder, and, when cold, broken up and ground in suitable mills. The best apparatus for grinding would be a large edge-stone mill. A series of toothed crushing rollers made of chilled cast iron answers very well. Common Buhr mill stones wear away too fast.

When drawing into water is deemed advisable, the workman should always make sure that the finished charge in the furnace contains no foreign saline matter visible as a limpid liquid amidst the viscous glass; for this thin liquor will produce violent explosions as it touches the water, though the silicate itself behaves very quietly.

The use of Glauber's salt instead of carbonate of soda in making window glass,—a substitution which was successfully carried out by Baader in 1808,—naturally suggests the employment of the alkaline sulphates for preparing waterglass. By some experiments made thirteen years ago with reference to this matter, I found that two equivalents of simple sulphate of soda required not less than three equivalents of sand for the complete expulsion of the sulphuric acid; and further trials made in the large way have gone to confirm this result. The decomposition of sulphate of soda is much less difficult when lime, alumina, or another sulphate is present. Thus one equivalent of sulphate of baryta, one eq. of sulphate of soda, two eqs. of carbon, and two eqs. of silica, melted easily to a perfect glass.—One eq. of sulphate of baryta, one eq. of sulphate of soda, two eqs. of coal, and three eqs. of sand, required a stronger heat.—One eq. of sulphate of baryta, two eqs. of sulphate of soda, three eqs. of carbon, and three eqs. of silica, fused easily to a clear glass.—One eq. of carbonate of lime, one eq. of sulphate of soda, one eq. of coal, and two eq. of silica were also vitrified with little difficulty. Feldspar,—$\dot{A}l\,\dot{K}\,\tilde{S}i_4$,—with the aid of carbon, decomposes nearly three eqs. of sulphate of soda, giving a very 'short' product which when powdered is readily attacked by acids, and might be used in the manufacture of alum.

The vitrification of sulphate of soda or potash, requires more heat, more time, and more skill than the production of waterglass from the carbonates; yet the far greater cheapness of the sulphates renders their use more economical. It is better to take purified sulphate of soda made by dissolving the crude article, precipitating the iron with lime, and evaporating the clear solution to dryness. This however involves time, labor, space, and extensive apparatus; while a silicate pure enough for ordinary use may be prepared directly from the clean residue of the nitric and muriatic acid manufacture. The iron, lime, and magnesia contained in the crude sulphate, do indeed somewhat impair the solubility of the product, yet when it is a sesquisilicate, it can be dissolved by hard boiling. If a little soda ash is added to the charge in the furnace, after the decomposition of the sulphate is finished, the subsequent solution is considerably facilitated.

The heat required is nearly or quite as great as that of an iron puddling furnace; and the melted materials act severely on

the interior of the furnace,—especially on the side walls just at the surface of the fused charge. A large allowance must therefore be made for 'wear and tear.' The furnace should be substantially built, the interior being constructed of very tough and compact fire bricks rich in alumina; and it would no doubt be advantageous to line the flue and the sides of the bed with some hard pressed and well burned blocks of the same material as glass pots. As economy of time and fuel requires a rapid consumption of coal, the grate surface must be large, say equal to one-fourth of the area of the bed, and the throat over the bridge should not be too contracted. To avoid the vexation of a leaking fire bridge, it is best to have the working bridge about four inches lower than the level of the grate. The sills of the charging and discharging doors may be on the same plane as the grate, and a gentle slope should connect the sill of the discharging door with the general level of the bed.* Were the doors not thus raised, the melted sulphate of the intumescent glass would flow out unbidden. I have used a furnace with a working sole 40 square feet in area, and a grate surface of 11 square feet. The chimney is about 50 feet high, and the arched flue leading straight into it, is 9 inches high in the centre, and 20 inches wide. In such a furnace four charges are worked in 24 hours, with a consumption of about 5000 lbs. of Pictou coal,—each charge consisting of 550 lbs. of white sand, 600 to 700 lbs. of crude sulphate of soda, and 70 to 90 lbs. of anthracite coal dust.† The approximate amount of carbon is best determined by actual trial. But on account of varying accidental influences no two charges are found to require precisely the same weight of coal. It is therefore best to reserve a few pounds to be thrown in afterwards, or not, according to circumstances. When the charge is well fused and the sand has all disappeared, if there is an excess of coal, the glass will remain black. If there is a deficiency of carbon, the mass will gradually become light colored,—as is shown by samples taken out from time to time on the end of an iron rod,—and on stirring, the sulphate will be seen to separate, as a thin liquid, from the pasty silicate. The workman should then throw in a pound or two of coal and stir well. The mass is now suddenly puffed up with the escaping sulphurous acid, but soon subsides again. A second or third addition of coal may be necessary, and some practice and tact

* The plan of having the bed lower than the grate may be highly recommended when there is a material to deal with, that becomes fluid in a strong heat. Thus I have found it to be very advantageous in smelting artificial sulphate of lead with iron borings and coal. A low sole necessitates a high arch, but when the arch slopes regularly downward from the highest point over the bridge to a low and wide flue, the consumption of fuel is found to be no greater than in a furnace of the more common form.

† When soda ash is used instead of the sulphate the same furnace affords six somewhat larger charges in 24 hours.

are required on the part of the workman that he may stop at just the right point. An excess of sulphate can be remedied, but an excess of coal is hard to get rid of. When the melted mass has become smooth and homogeneous and light colored, it may be decolorized with arseniate of soda and drawn out, the hoes being changed as they get hot and pliant.

For making a liquid or pasty silicate, the glass coarsely ground may be boiled with water till the solution stands at about 25° B. after cooling. If it is made much stronger it will not settle readily. Some dissolve waterglass by blowing steam directly into the water, but in that case the solution goes on very slowly, because the heat is insufficient. It is far better to have an iron kettle heated by a fire. After being allowed to deposit the insoluble matter, the liquid may be concentrated by evaporation to any required degree within certain limits. When it becomes thick, the further application of heat is attended with no little difficulty; for the silicate then rapidly adheres to the kettle, and there is needed very diligent scraping with a chisel-pointed bar to keep the bottom and sides of the vessel clear of the spongy coating. And to push the evaporation to dryness, is quite out of the question. Sesquisilicate of soda cannot conveniently be made stronger than 50° B.

When we wish to obtain the greatest possible relative amount of silica in solution, it is necessary to make the glass of none but the purest materials. Earthy or metallic oxyds very much lessen the solubility of the product, and if more than a mere trace of them is present, a larger proportion of alkali is required to render the mass capable of yielding readily to boiling water. A silicate so contaminated will not in fact enter directly into solution; it is only decomposed by boiling with water, into a more alkaline silicate which dissolves and a compound earthy silicate which remains as a bulky residue, often in the form of plates or scales. Thus of a well worked waterglass made from crude sulphate of soda, water took up only 89 per cent, leaving a copious sediment consisting of soda, lime, magnesia, alumina, ferrous and ferric oxyds, and silica. Indeed common glass is but an alkaline silicate rendered insoluble by a more considerable amount of lime or oxyd of lead. Fuchs himself pointed out the necessity of having the sand free from lime and alumina. He states that a little iron does no harm; but this can be true only when the glass is left brown or black so that the iron exists in the state of sulphid. Fuchs says that with pure quartz and pure potash an insoluble glass cannot be prepared. "For if we take two parts of quartz to one of potash, we obtain,—as I have convinced myself,—a glass which partly dissolves in water." This would make about $\dot{K}_2\ddot{S}i_{10}$. "Besides even glass containing lime is more or less attacked by boiling

water, as has been long known, and as Scheele especially proved. I found that many a glass, when it is rubbed a long time with water in an agate mortar, reacts very sensibly alkaline, and that when finely powdered glass is boiled for many hours with water, a fluid is obtained which has an alkaline reaction and gives a flocculent precipitate with sal ammoniac." Pelouze found that when good white glass,—$\dot{N}a_8\ \dot{C}a_{11}\ \ddot{S}i_{32}$,—was finely pulverized and boiled for some time with water, about three per cent was dissolved. Another kind, containing a less proportion of lime, ($\dot{N}a_4\ \dot{C}a_2\ \ddot{S}i_{13}$,) yielded to boiling water 18·2 per cent, and what entered into solution proved to be sesquisilicate of soda.* And Fresenius affirms that even a glass vessel yields a ponderable amount of its substance to water which is boiled in it only a short time. Still a simple potash or soda silicate with three equivalents or more of acid to one of base, may be considered as practically insoluble. Thus Peligot found the so-called 'alabaster glass' to consist almost entirely of silica and potash in such proportions as to make $\dot{K}\ \ddot{S}i_5$, were the silica all in combination. But in this case a part of the silica is merely diffused, and so produces the opacity of the glass. Stein however found besides, 1·5 per cent of lime and 2·3 per cent of bone earth, in alabaster glass.†

Analysis.—When a silicate is in lumps or coarse powder, we can judge as to the absence of foreign salts by the degree of transparency. A well made article is clear, bright and homogeneous. One not properly prepared has a dull, resinous appearance; and if there is much sulphid or chlorid present, the mass will be spotted, streaked, cloudy, or entirely opaque. Less than ten per cent of intermixed saline matter, is sufficient to render waterglass quite milky.

But the real value of any sample can be ascertained only by a chemical examination, and as the analysis is liable to some peculiar chances of error, it may be well to point out a few of the peculiar precautions to be observed.

In the case of a dry glass, especial pains must be taken to insure the solution of everything that water will take up,—a matter requiring some labor and time. For determining the amount of alkali, I take an average of the specimen to be tried and patiently rub some, in a very hard, four inch Wedgewood mortar, to an exceedingly fine powder,—grinding only about a gram at a time, and being careful to keep it as dry as possible. Five grams or more are then weighed out and boiled with forty or fifty times as much water, in a porcelain dish, for a time, varying from fifteen to ninety minutes, as occasion may require. To prevent a violent bumping, the mixture must be stirred

* Liebig and Kopp's Jahresbericht for 1856, p. 354.
† Wagner's Jahresbericht,—iv, p. 244.

briskly and without intermission, during the whole time of heating. If there is no uncombined sand in the article under treatment, the cessation of the gritty feeling, shows plainly when a farther application of heat is unnecessary. For a well fluxed sesquisilicate of soda, 15 minutes boiling is quite sufficient. Any uncombined sand present in a sample, causes an uncertainty in the time required. Should there be a gritty residue after half an hour's boiling, it is advisable to stop and make a preliminary testing. If we find less than 28 per cent of alkali indicated, it is best to start anew and heat at least an hour and a half.

After the solution is completed, the whole should be made up to a given weight,—being at least forty times that of the dry silicate,—covered, and allowed to get perfectly cold. The larger part of the liquor being then poured off, without disturbing the sediment, a suitable proportion of the whole amount may be weighed out and tested by any of the common alkalimetric methods. Filtration is tedious and unnecessary, for though the decanted liquid is seldom perfectly clear, even after long repose, the quantity of suspended earthy matter is altogether too insignificant to influence the correctness of the result. Waterglass which is already in the liquid or pasty state, must be largely diluted before trial, since a strong or a warm solution is apt to gelatinize before all the acid is added, and then exactness is impossible, because the thick jelly is not readily penetrated by the acid subsequently dropped in. Therefore should gelatinization occur, a new trial must be made with a weaker or colder solution.

When the operation is finished, if sulphuric acid has been used, the tested liquid itself can be dried down, and then the silica is left in a coarse granular form, easy to wash, collect, ignite and weigh. If the precipitation of the silica is effected with an ammonia salt, the residue is likely to be very bulky, and so fine and light that it requires no little care to keep it from blowing partly away during ignition.

An excess of nitric or chlorhydric acid added to very dilute waterglass, causes no apparent change for a long time, and in a weak solution so treated we may test for sulphates or chlorids, for iron or for arsenic acid, without previously removing the silica. For the quantitative determination of sulphate and chlorid it is best to treat the silicate solution with an excess of nitrate of ammonia instead of an acid, and thus prevent any loss of chlorine in drying down.

If sulphids are present in a liquid waterglass, a small bit of sulphate or carbonate of lead dropped in will soon become discolored.

When the per-centage of water is to be ascertained, it is not well to heat the solution by itself, for in such a case it finally expands to an enormously bulky, vesicular, unmanageable mass.

A known quantity of freshly ignited sulphate of lime should first be stirred in, the weight of the sulphate being at least twice as great as that of the dry alkali supposed to be present. Then in a few moments a double decomposition is effected and the glutinous nature of the silicate is destroyed. The whole becomes stiff and crumbly, and the water may be driven off with great ease, while everything except the water is retained.

As all waterglass contains in combination more or less of the earthy silicates which remain as a light sediment after dissolving, the amount of unchanged sand can be found only by resorting to a mechanical separation. For this purpose a quantity of the coarsely ground silicate should be boiled with water till everything soluble is taken up, and then the liquor being stirred and allowed to settle for a moment, may be decanted, carrying with it the suspended silicates. The gritty residue left after one or two washings conducted in the same way, represents pretty nearly the amount of uncombined silex.

[To be continued.]

ART. XVII.—*Sketch of the Distribution of Forest-trees in Nebraska Territory;* by JAMES T. ALLAN. (In a letter to Prof. GRAY.)

THE relative proportion of the several species and their general distribution in the tract of country lying west of the Missouri and between the paralells 40° and 43° is all that I will attempt to describe in this sketch.

And one remark will apply to all this country, that timber is found only upon streams or small groves about some spring on the wide prairie.

The valley of the Missouri is from three to six miles wide, and sometimes the stream winds down near the centre with both sides fringed with willows, behind which is a belt of cottonwood (*Populus monilifera*); these trees often 80 to 100 feet high: upon the bluffs which wall each side of this valley we find the different varieties of hard wood, also upon the hills, and ravines opening toward the river. On the tops of these hills we find *Quercus alba* and *rubra*, with occasional trees of *Quercus coccinea;* half way down the steep sides of these ravines we find *Tilia Americana* and *Ulmus fulva* in about equal quantities, with clumps of *Carpinus Americana*. Still lower down and in the rich soil at the bottom are *Gymnocladus Canadensis*, *Celtis occidentalis* and *Fraxinus Americana*, while on the cool northern slope half hanging down the hill are plenty of *Staphylea trifolia* and *Rubus occidentalis*. As we recede from the river toward the summit of the ridge we find scattering trees of *Carya alba* among the Oaks before mentioned, till we come to the prairie where the trees

terminate with a few "Scrub Oaks," 12 to 20 feet high, standing beyond the fringe of *Corylus Americana*. Among the latter in the spring we discover the bright blossoms of the Red bud (*Cercis Canadensis*) and in autumn the bright seed-pods of the *Euonymus*.

Going west from the Missouri we find no trees except on the small streams on which, at intervals of ten miles or more are groves of White oak, Bur oak, *Carya glabra* and sometimes White elm; these groves will average in extent from 100 to 500 acres, and the above mentioned, with *Juglans nigra*, make up their prominent trees.

The valley of the Platte, to which so much attention is now directed as the great central route, demands a notice. At the mouth we find a heavy body of timber, chiefly Cottonwood, with a small proportion of *Acer rubrum* and *Morus rubra*. As we proceed up 20 miles the dull green of *Juniperus Virginiana* begins to be seen, which farther up we see covering some of the small islands, with trees often 18 inches and more in diameter. This has furnished for a hundred miles or more telegraph poles of a superior quality.

Upon the tributaries of the Platte, particularly on the north side *Quercus macrocarpa* is the most abundant of large trees.

After leaving Fort Kearney, the immigrant finds but a fringe of cottonwoods skirting the stream, and on the road to the new gold mines for 200 miles not a tree for shade or wood: the well known "Buffalo chips" must supply the latter. Upon the North Platte the emigrant to California or to the Mormon "Zion" is always in sight of the trees bordering on the stream, though often too far off to obtain fuel. 400 miles beyond Ft. Kearney is found in several places large groves of *Negundo aceroides* on the banks of the creeks.

After passing Fort Laramie the pleasant sight of green pines, though at a distance, relieves the eye and tells of the cool waters in the south pass, so refreshing in the heat of summer.

The very rapid growth of trees in this rich soil is a noticeable feature. The hazel which fringes the timber on the prairie side is interspersed with abundance of saplings *Carya alba* and *glabra* and *Alnus fulva*, which shoot up with wonderful rapidity, while upon the sand bars of the river, as soon as the waters subside in July, there spring myriads of young cottonwoods and willows. Of the latter I have neglected to speak though they cover no inconsiderable portion of the valleys of the Missouri and Platte. Everywhere fringing the streams, and where there is a tract annually overflowed by the spring rise there we find a dense growth often 20 to 30 feet high and from one to three inches in diameter growing so thickly that it is impossible without great difficulty to force a passage through them.

The relative proportion of the several species may perhaps be set down as follows:

1. *Populus monilifera.*
2. *Quercus macrocarpa.*
3. *Quercus alba* and *Quercus rubra.*
4. *Tilia Americana, Ulmus fulva* and *Quercus discolor.*
5. *Juglans nigra, Ulmus Americana.*
6. *Carya alba, Carya glabra.*
7. *Fraxinus Americana, Celtis occidentalis.*
8. *Juniperus Virginiana, Platanus occidentalis.*
9. *Acer rubrum, Gymnocladus Canadensis.*

Besides the above mentioned, of the smaller varieties there are *Prunus Americana, Zanthoxylum Americanum, Staphylea trifolia, Negundo aceroides, Corylus Americana, Carpinus Americana, Alnus incana, Euonymus Americana, Cercis Canadensis, Cornus sericea ?*

In the Omaha land district, which contains something like 4000 square miles of land, there appears from the plots in the office to be about 75,000 acres of timber. A tract of country of equal size lying west of it would contain much less.

The botanical names above correspond to and have been compared with the descriptions in Gray's "Manual."

Omaha, Nebraska, April 2, 1861.

ART. XVIII.—*Remarks on the Age of the Goniatite Limestone at Rockford Indiana, and its relations to the "Black Slate" of the Western States, and to some of the succeeding rocks above the latter;* by F. B. MEEK and A. H. WORTHEN, of the Illinois State Geological Survey.

IT is known to most of those who have studied the geology of the west, that there is an outcrop of limestone near Rockford, Indiana, usually termed the Rockford Goniatite bed, in regard to the age of which there is some difference of opinion amongst geologists. This rock is only exposed at a single locality in the bed of a small stream, where not more than a thickness of two feet of it is seen. It is a mottled brownish and ash-colored argillaceous limestone, and contains, in addition to the *Goniatites* from which it takes its name, other fossils belonging to the genera *Nautilus, Orthoceras, Pleurotomaria, Euomphalus, Spirifera, Rhynchonella,* &c.

No other rock is seen above or below this at the Rockford locality, but in sinking wells in that vicinity it has been ascertained that it rests upon a Black Slate forming a marked horizon

in several of the Western States. Owing to the fact that this slate has been seen to form the bed the same stream at another place about one mile above, and at a slightly higher elevation, it has been supposed that the Goniatite bed is intercalated in it, and the mystery has been that the fossils occurring in the limestone have a decidedly Carboniferous aspect, and in some instances have even been considered actually identical with well known European Carboniferous species; while the Black slate has been referred by Prof. Hall of Albany, New York, to the horizon of the Marcellus shale of the N. Y. series, occupying a position far down in the Devonian, at the base of the Hamilton Group.* In addition to this, Prof. Hall also refers a rather extensive series of fine arenaceous, and more or less argillaceous deposits, holding a position above the Black slate near Louisville, Ky., and at other localities in the west, to the Portage and Chemung Groups of New York, higher members of the Devonian, which are in New York overlaid by a great thickness of upper Devonian strata equivalent to the Old Red Sandstone, of the British geologists.

Without attempting to give a detailed statement of the opinions that have from time to time been expressed in regard to the age of the Black slate and overlying strata alluded to, we would remark that the most generally received opinion amongst western geologists has been, that the whole series known in Indiana and Kentucky as the "Fine-grained sandstone of the Knobs," down to the Black slate, should be included in the Carboniferous system, and some even include the latter also in the Carboniferous.

When the distinguished French geologist, DeVerneuil, was in this country in 1846, he made an excursion through the Western States for the purpose of studying our rocks, and obtained a fine collection of their characteristic fossils. After his return to Europe he published a highly interesting memoir on the parallelism of American and European formations, in which he referred all the fine arenaceous and shaly beds holding a position between the Black slate and the Carboniferous limestones in Indiana, Kentucky, and Tennessee, as well as a portion of the Waverly sandstone in Ohio, to the Carboniferous system. One of the *Goniatites* given to him by Dr. Owen from the Rockford limestone, he considered identical with *G. rotatorius* of Koninck, a well known Lower Carboniferous species, which led him to refer the limestone from which it was obtained to the lower Carboniferous. The Black Slate, however, he referred to the horizon of the Genesee slate of the New York series.†

* See Proceed. Am. As. Geol. and Nat., p. 267, vol. i; also Rept. 4 Dist. N. Y. Geol. Survey, p. 519.

† See Bulletin Geol. Soc. France, vol. iv, 2d Series.

After the publication of this paper, Prof. Hall was for a time inclined to concur, at least in part, with De Verneuil's views, but he subsequently returned to his former opinion, which he has continued to maintain, with his usual zeal and ability, to the present time. In an important paper recently published by him in the 13th Annual Report of the Regents of the University of New York, he describes a number of fossils from the Rockford Goniatite bed, along with others from the Marcellus shale in New York, and refers not only the Black slate, but the Rockford limestone, which has been supposed to be intercalated in it, to the horizon of the Marcellus Shale.

As the question in regard to the age of the Rockford goniatite bed has an important bearing on the parallelism of our rocks with established horizons elsewhere, we have, while investigating the fossils in the Illinois State Geological collection, carefully compared them with a series of specimens from Rockford, Indiana, in order to determine whether or not the Goniatite bed at that place is represented in our state, and if so, what relations it bears to our other rocks. These comparisons have led us to the conclusion that it is represented in Illinois, as well as in Missouri and Iowa, and that it holds a much higher stratigraphical position than that assigned it by Prof. Hall.

In the first place, we should remark that there is no evidence whatever, that the rock under consideration is overlaid at Rockford, or at any other locality, by any part of the Black slate, though we know the slate occurs beneath it there, as well as in Illinois.

Again after a careful study of a series of the Rockford fossils, we have clearly satisfied ourselves that the limestone from which they were obtained is of the same age as the Chouteau limestone of Prof. Swallow. The evidence of this is the positive identity of at least six of the 23 or 24 known Rockford species, with characteristic forms of the Chouteau limestone in Missouri and Illinois, while most of the others are either identical or closely allied. The following is a list of the species from Rockford which are known to be identical with Chouteau limestone species found in Missouri and Illinois, viz:—*Nautilus digonus*, Meek and Worthen, *Euomphalus lens*, Hall, *Rhynchonella Missouriensis*, Shumard, *Spirifera Cooperensis*, Swallow, (=*S. semiplicata*, Hall) *Cardiopsis radiata*,* Meek and Worthen, (=*Megambonia Lyoni*, Hall,) and *Sphenopterium enorme*, Meek and Worthen.

* We placed this species at first, provisionally, in the genus *Cardiomorpha* of Koninck, but suggested at the same time that we suspected it to be generically distinct. Later examinations have satisfied us of the correctness of this suggestion, and we have proposed to establish for its reception a new genus, under the name of *Cardiopsis*, from its resemblance to *Cardium*. It has no near relations to the type of the genus *Megambonia*.

In classifying the rocks of Missouri, Prof. Swallow, (and his assistants, including one of the writers) knowing that his Chouteau limestone holds the same position, and contains many of the same fossils found in beds elsewhere in the west referred by Prof. Hall to the Chemung group, placed it on a parallel with the Chemung, and Prof. Hall has also since referred equivalent beds in Iowa to the same horizon.

In order that the reader may understand more clearly the position of the Chouteau limestone with relation to our other western formations, we give below a section showing the order of succession of the several beds, beginning with the Burlington limestone, which is acknowledged by all to be Carboniferous, and extending down to the Hamilton Group inclusive:—

1.—Burlington limestone attaining a thickness of -	200 feet.
2.—Chouteau limestone, - - - -	100 "
3.—Vermicular sandstone and shale, - -	65 to 100 "
4.—Lithographic limestone (rather local),	60 "
5.—Black Slate, - - - -	30 to 40 ft.*
6.—Hamilton group, - - - - -	120 "

Numbers 2, 3, and 4 of this section were included by Prof. Swallow in the Chemung, though Prof. Hall thinks that at least a part of the Lithographic limestone, should be referred to the Hamilton group. It is true a few of the many fossils found in the Lithographic limestone resemble Hamilton forms, and one of them seems to be undistinguishable from *Orthis Vanuxemi*, of Hall, a common Hamilton group species. It is well known, however, that many of the other fossils found in this limestone occur in the Chouteau beds above, where they are sometimes mingled with Carboniferous forms. At the same time, unless the *Orthis* just mentioned is an exception, not one of the species occurring in the Lithographic limestone and the Chouteau beds above, can be, so far as our knowledge extends, positively identified with any of the numerous well marked Hamilton forms in the beds below the Black slate.

In regard to the occurrence of the *Orthis* which seems to be undistinguishable from *O. Vanuxemi*, in the Lithographic limestone, we would remark that we do not think it a fact entitled to much weight, when it is borne in mind that *O. Vanuxemi* is so very closely allied to the Carboniferous *O. Michilini* of L'Eveille, that even Mr. Verneuil regarded them as scarcely distinguishable, and the differences between them have not yet been clearly defined. Prof. Hall acknowledges that it "is so closely allied to *O. Michilini* of L'Eveille, that it is

* In some parts of Ky. and Indiana, the Black slate attains a thickness of about 100 feet.

very difficult to point out characters by which it may be distinguished." He thought, however, that, judging from DeKoninck's figures of *O. Michilini*, he could see some slight differences in the vascular impressions and dental processes, and remarked that the "minute granulations or punctæ upon the exterior surface [of *O. Vanuxemi*] present characters which are not noticed in the figures and descriptions [of *O. Michilini*] so far as I have observed."* Yet we find in Mr. Davidson's very carefully written description of *O. Michilini*, since published in his Monograph of the Carboniferous Brachiopoda of Scotland, that he says the surface is "covered with minute punctures." In fact almost every word of Mr. Davidson's description would apply equally well to *O. Vanuxemi;* and after a very careful comparison of our western form with authentic specimens of *O. Michilini* from Scotland, sent by Mr. Davidson, we have been unable to find any appreciable differences. We are not, however, contending that they really are identical, but we simply mean to say that it is unsafe to base conclusions upon such closely allied species.

The three subdivisions Prof. Swallow has called Vermicular sandstone and Shale, and the Chouteau limestone above, although often distinctly separable by their lithological characters, seem sometimes to replace each other, or often one is augmented in thickness at the expense of the other, though they are mainly characterized by the same fossils. The whole group is very variable in its lithological characters, the very same beds that are at one place composed of hard bluish or ash-colored compact limestone, being at others a yellowish very fine soft argillaceous, or arenaceous rock, or consisting of alternations of brownish and greenish shaly limestone, &c. The fossils are usually in a better state of preservation in the limestones than in the other beds, and assume a more decided Carboniferous aspect than in the arenaceous and argillaceous strata, even where the latter occur above the limestones.

The Black slate, so far as our knowledge extends, seems to be nowhere greatly developed in Missouri, though it is probably represented at Hannibal in that state by six feet of blue shale beneath the Lithographic limestone, as may be seen by Prof. Swallow's Section No. 15, p. 99, of the Missouri Report. Some eight or ten miles east of Hannibal, in Illinois, it is seen occupying the same position with relation to the Lithographic limestone, and attaining a thickness of thirty or forty feet.

Although the position of the well marked Hamilton beds with relation to the representative of the Black slate, have probably not been very clearly determined in Missouri, they are there

* Tenth Ann. Rept. Regents University of N. Y., p. 136.

known, as Prof. Swallow has shown, to hold a position beneath the Lithographic limestone. In Illinois, however, numerous exposures show that the black slate comes in just above all the well defined Hamilton Group beds as may be seen by the following section taken near Jonesborough, Union county, Illinois:—

1. Brown silicious shale probably representing the Vermicular sandstone and Shale of Prof. Swallow, 50 to 60 ft.
2. Black Slate with its characteristic *Lingula*, 40 to 50 ft.
3. Hamilton group, consisting of a dark limestone, containing *Phacops bufo? Strophomena Hamiltonensis, Trepidoleptus carinatus, Heliophyllum Halli*, and other well known Hamilton species, along with *Atrypa reticularis*, &c., 120 ft.

Now as the Chouteau limestone of Prof. Swallow, is known to hold a position immediately beneath the Burlington limestone, and considerably above the horizon of all the Hamilton group beds of the West, as well as above the Black slate, which also overlies the Hamilton group in Illinois, it follows as a matter of course, that its representative in Indiana, the Goniatite bed at Rockford, cannot belong to any part of the Marcellus shale, lying at the base of the Hamilton group.

Again, the fact that all our well marked Hamilton beds in Illinois, hold a position beneath the Black slate, while in New York, as already stated, the Marcellus shale lies at the base of the Hamilton Group, renders it very improbable that even our western Black slate formation represents the Marcellus shale. Its position would seem to be more nearly that of the Genesee slate of New York, as suggested by M. DeVerneuil, which supposition is strengthened by the affinities of the only fossils yet found in it, viz,—a small *Lingula* and a *Discina*, scarcely, if at all, distinguishable from species occurring in the Genesee slate of New York, to which in fact they were referred by Prof. Yandell and Dr. Shumard in 1847.

Although Prof. Hall describes about twenty species of fossils from the Rockford *Goniatite* bed, it will be observed that he does not identify any of them, with Marcellus shale species, or with forms occurring in any other part of the Hamilton group either in New York or in any of the Western States; but refers the rock in which they occur to the horizon of the Marcellus shale, solely from the fact that it was thought to be intercalated in the Black Slate, and from the supposed affinities of its *Goniatites*, to Marcellus species. Aside, however, from the evidence we now have that the Rockford limestone is of the same age as the Chouteau limestone of Missouri, which comes in some distance above

the Black slate, and is exactly equivalent to beds referred by Prof. Hall at Burlington, Iowa, to the Chemung, its whole group of fossils including the *Goniatites*, strike us as being more nearly allied to Carboniferous forms than to those of any of the New York rocks.

If we take for instance the three species of *Goniatites* now known from the Rockford bed, we find first that *G. Lyoni*, Meek and Worthen, (=*G. hyas* of Hall,) which Prof. Hall compares with *G. Chemungensis*, of Vanuxem, differs materially from Vanuxem's figure of that species, in having but two lobes in each septum visible on either side, while *G. Chemungensis* is figured as having five. It bears no near analogy whatever, to any of the species figured by Prof. Hall, from the supposed equivalent bed in the Marcellus shale of New York.

Again *Goniatites Oweni* of Hall,* is much more closely allied to *G. princeps* of Koninck, (a European Carboniferous species) than it is to any of those yet known from the Marcellus shale, or any other rock in New York. It agrees in size and form with Koninck's species, as well as in having constrictions at intervals left upon its internal cast by the occasional thickening of the lip, while it presents but slight differences in the lobes of its septa; the most obvious difference being in the length of its dorsal lobe, which is more attenuated than in *G. princeps*, though even in this character, the Rockford specimens vary to some extent. It is true *G. princeps*, as figured by Mr. Koninck, is marked by regular costæ, while *G. Oweni*, as we usually see it in the condition of worn casts, seems to be smooth. We have, however, seen unmistakable traces of similar costæ, on some of the Rockford specimens. We are not contending, however, for the absolute specific identity of these forms, but we do maintain that the analogy of the Rockford species to *G. princeps* is much nearer and more striking than its relations to any of the Marcellus shale species.

The third species, *Goniatites ixion* of Hall, while it presents but very remote analogy to any of the Marcellus shale species, is scarcely, if at all distinguishable from the Carboniferous species, *G. rotatorius*, of Koninck,† with which Mr. Verneuil considered it identical. They may possibly belong to different species, yet the minute differences pointed out by Prof. Hall, are, we think, not inconsistent with specific identity in a genus like this. Every one who has studied the *Ammonitidæ* must be aware how unsafe it is to base specific distinctions on minute differences in the smaller details of the lobes of the septa, or upon slight differences in the size of the umbilicus. We are

* See thirteenth Ann. Report Regents University New York, p. 100.
† Animaux Foss., pl. 51, fig. 1.

not insisting, however, upon the exact identity of these forms, for that is a question that can only be settled by direct comparison with authentic specimens of the European species; yet that they are at least strikingly analogous, must be evident to any one who will but glance at Prof. Hall's, and Prof. DeKoninck's figures.

If we compare the two species of *Nautilus* now known, from the Rockford beds, *N. digonus*, and *N. trisulcatus*, Meek and Worthen, we find them wholly unlike any forms known in the Marcellus shale, while the latter species is allied to the Carboniferous *N. sulcatus* of Sowerby, (see Koninck's An. Foss., pl. xlvii, fig. 10), and the former is analagous to two or three of the species figured by DeKoninck from the Carboniferous rocks of Belgium. In fact both of these species belong to a peculiar subgenus of *Nautili* for which we have proposed the name *Trematodiscus*, a group embracing a number of discoid Carboniferous species, with a wide perforated umbilicus and narrow whorls, which are ornamented with longitudinal angular ridges, and sometimes with parallel striæ. So far as we know, the entire group is confined to the Carboniferous system, unless those found in the Rockford *Goniatite* bed and its equivalent in Illinois, are exceptions.*

In order to show the close relations between the Chouteau limestone and the Burlington (acknowledged Carboniferous) beds above, in Illinois, we might give many sections, with lists of fossils, but the following taken near Grafton in Jersey county, will be sufficient for illustration:—

1.—Burlington limestone,
presenting its usual characters and containing its usual fossils; showing at the immediate outcrop where the section was taken, a thickness of 8 to 10 feet, but increasing as the country rises back to a thickness of 150 or 200 feet.

2. Chouteau limestone.
Upper part consisting of ash-colored argillaceous shelly limestone in thin lares, with greenish marly partings and calc spar concretions. Containing *Cyathoxina* (undt. sp.), *Poteriocrinus Meekinus*, *Actinocrinus* (undt. sp.), *Spirifera peculiaris*, *S. grimesi*, *Athyris Prouti*, *A. Hannibalensis*, *Strophomena rhomboidalis*, *Orthis Michilini*, *O. resupinata*, *Productus semireticulatus? Platyceras equilatera*, - - - - - - - - - 40 feet.

* The forms included in this subgenus differ so widely from the recent typical species of *Nautilus*, that few conchologists would place them in the same genus if they were found living in our present seas. It includes such species as *N. pingius*, *N. stygialis*, *N. Edwardsianus*, and *N. Omalianus* of Koninck, and *N. sulcatus*, and *N. cariniferous* of Sowerby. *Gyrocerus gracile* of Hall, from the Rockford limestone probably also belongs to this group.

Lower beds, composed of brownish-buff colored argillaceous limestone, with nodules of calc spar, the upper part being in thicker beds than the lower which has a more greenish tinge. Fossils—*Strophomina rhomboidalis, Chonetes ornata, Cardiopsis radiata, Productus semireticulatus? Cyrtoceras* and *Gomphoceras* (undt. sp.) *Nautilus digonus*, a discoid *Goniatite*, and a *Proetus* (undt.) - - 60 feet.

3.—Black slate shading upwards to greenish shale, - - - 40 "

4.—Hamilton group, consisting of dark limestone, - - 10 "

5.—Niagara group limestone of buff col., - - - - 100 "

Of the fossils mentioned in the above section the following are usually characteristic of the Chouteau series:—*Cyathoxina* (undt.), *Poteriocrinus Meekianus.** *Spirifera peculiaris, Athyris Prouti, A. Hannibalensis, Chonetes ornata, Cardiopsis radiatus, Nautilus digonus*, and an undetermined *Goniatite*. Those usually regarded as strictly Carboniferous are,—*Spirifera Grimesi, Orthis Michilini, Productus semireticulatus*, and *Platycerus equilatera*.

The forms common to Carboniferous and Devonian rocks, are *Strophomena rhomboidalis*, and *Orthis resupinata*. The species here enumerated, however, are only those collected at the locality where this section was taken. Several other Carboniferous forms are sometimes found in the Chouteau beds at other places, and a number of other Chouteau species are known to range up into the Burlington beds. From a locality in an adjoining county, we have an *Actinocrinus*, from the Chouteau series, which we can only distinguish as a variety, from the well known Burlington *A. pyriformis* of Shumard; and along with it we also find a species of a peculiar coral (*Sphenopoterium enorme*) described by us from the Rockford Goniatite bed.

In an interesting and important paper on the rocks and fossils of Burlington, Iowa, published by Mr. Charles A. White of that place, in the Boston Journal of Natural History, (vol. vii, p. 209) he shows that out of a list of one hundred and two described species occurring in the Burlington limestone, fifteen commenced their existence in the beds below, referred by Prof. Hall to the Chemung; which as is well known, represents the Chouteau limestone of Prof. Swallow.

When we thus find these beds so intimately connected by the general affinities of their organic remains, as well as by the actual mingling of species with the undoubted Carboniferous rocks above, and bear in mind that the Chemung group in New York and Pennsylvania, is surmounted by another Devonian formation, (the Old Red Sandstone) between two and three thousand

* At the time Dr. Shumard described this crinoid, its exact geological position was not known, the specimen having been found loose at the base of a bluff composed in part of the Chouteau beds, and partly of the Burlington limestone. Its true position has since been determined to be in the Chouteau limestone, though it probably also ranges up into the Burlington limestone in some places.

feet in thickness, the questions naturally suggest themselves,—are we right in referring these Chouteau beds to the horizon of the Chemung? Can it be possible that a great formation like the Old Red Sandstone, characterized by the remains of a peculiar fauna of its own, is wanting here between the Chouteau and Burlington limestones?

But on the contrary, it may be said, if we do not adopt this conclusion, what are we to do with the fossils occurring in these rocks apparently identical with Chemung species? In reply to this latter question, we would state that although we have for a long time thought a few species occurring in the Chouteau beds identical with Chemung forms, later comparisons with New York specimens from the latter formation, lead us to doubt their actual specific identity. They are generally obscure casts of *Avicula* and other lamellibranchiate mollusca, some of which also resemble Carboniferous species. Admitting, however, that they are undistinguishable from Chemung species, does their presence here in the same beds with numerous other fossils totally distinct from Chemung forms, and not only more nearly allied to Carboniferous species, but in some instances identical with those occurring in acknowledged Carboniferous strata above, warrant us in placing these beds on a parallel with the Chemung group?

Mr. White, in discussing this question in his paper already cited above, says, "admitting that some of the species found in the lower beds have been identified with those of the Chemung group of New York, it settles beyond question their geological equivalency, but does not necessarily prove that they were contemporaneous. Indeed, it seems probable that they were not so, by an interval of time that it would take the species to migrate that distance. May it not, therefore, be inferred that the species originated at the east, and were migrating westward during the time that the bottom of the Chemung seas was sinking, and receiving upon it the deposit of the Old Red Sandstone,—thus making these Devonian rocks equivalent to the Chemung of New York, and contemporaneous, at least in part, with the Old Red of the Katskill Mountains?"

If any of the species found in the strata under consideration are really identical with Chemung forms, as has been supposed, Mr. White's suggestion that they had prolonged their existence by migrating westward, until long after the close of the Chemung epoch in New York, is not improbable, but in that case, we should not refer the rock in which they occur to the Chemung, but either to the Old Red, or to the Carboniferous, for in using any of these names we refer to a period in time, as well as to a group of strata. The entire group of fossils, however, found in these beds is far more nearly allied to the Carboniferous fauna than to that characterizing the Old Red either in this country or

in the Old world; in short we know of nothing in this formation bearing any analogy to the fossils of the Old Red sandstone.

In conclusion, we would remark that the relations between the Chouteau and Burlington limestones in Missouri, Iowa, and Illinois, where both occur together, as well as the affinities of the fossils found in the former in the states mentioned, and at Rockford, Indiana, show that it should probably be referred to the Carboniferous system; or, at any rate that it is much more recent than the Chemung, and not equivalent to any New York rock.

It is not improbable, however, that the Portage and Chemung groups may be represented between the Chouteau limestone and the horizon of the Black Slate, at some places in the Western States alluded to, but if so, their presence there has yet to be clearly demonstrated.

Art. XIX.—*Chemical Contributions;* by M. Carey Lea.

1. *On the Preparation of Nitrate and Nitrite of Ethyl.*

Nitrate of Ethyl.

The tendency of alcohol to decompose nitric acid is so great that while we have any number of processes for obtaining nitrite of ethyl, Millon's is the only one known for forming the nitrate. But Millon's process fails much oftener than it succeeds (such at least has been my experience) in consequence of the proportion of urea directed to be used being much too small.

Millon's process as quoted in Kolbe's Organische Chemie, directs to distill equal weights of alcohol and of colorless nitric acid sp. gr. 1·401. Not to operate upon larger quantities than 120 to 150 grammes, and to add a little urea, to the above quantity, at most ("höchstens") 2 grammes of urea.

Conducted as above, the operation succeeded with me but once out of four or five times. But by using a large quantity of urea, three or four times as much, or even more, success invariably follows, and there results a great saving of urea, because when the process fails, the materials, urea included, are entirely lost, whereas when conducted as just described, very little urea is consumed. It is only necessary to add fresh acid and alcohol, after the first proportion has been distilled to about one fifth, and the process may be repeated almost indefinitely, with the occasional addition of a little urea to keep up the proportion. The product in the second and subsequent operations is much larger than in the first.

Nor have I found it necessary to operate on the small quantities above mentioned, but have habitually used quantities

amounting to 400—500 grammes, and should not hesitate to employ still larger ones. It is however more convenient to use the above quantity, and replace the material as fast as exhausted.

The facility with which nitrate of ethyl can be obtained by the above process must, I think, give it a decided advantage over the iodid of ethyl for the preparation of the ethyl bases, in the manner which I described in the last number of this Journal. The lower equivalent of nitric acid as compared with iodine is a matter of considerable importance, one hundred parts of iodine are capable of holding in combination something less than 23 of ethyl, whereas one hundred parts of nitric acid combine with over 53 of ethyl; a material point as the iodine on the one hand and the nitric acid on the other are merely the vehicles for bringing about the ethyl-substitution. In operations in pressure tubes where space is valuable, this is an important consideration; in an economical point of view also, the advantage appears to be greatly on the side of the nitrate of ethyl.

NITRITE OF ETHYL.

The action of nitric acid on alcohol in the preparation of nitrite of ethyl may be conveniently moderated by protosulphate of iron, and the nitric acid reduced by that agent instead of at the expense of part of the alcohol. The proportions which I have used are, nitric acid sp. gr. 1·37, 90 cc., alcohol of 90 per cent, 150 cc., ferrous sulphate 45 grammes. The product is very large, the distillate on being shaken up with water yields more than half its bulk of nitrite of ethyl. The latter is not perfectly free from aldehyd, but more so than that obtained by some other processes. The action is perfectly quiet to the last, and the distillation may be carried on rapidly.

2. *On the Effects of Reducing Agents upon Nitrite of Ethyl.*

As ammonia under the influence of oxydizing agents is burned to nitrous and nitric acids, so these acids when acted upon by reducing agents, might naturally produce ammonia, or in the presence of a decomposing ethyl compound, might give rise to the formation of substituted ammonias. The experiment however seems to prove that while ammonia is a constant result of this reduction, it varies greatly in quantity according to the particular reducing agent employed, and that substituted ammonias are not produced, at least not in recognizable quantities. The following were the reactions observed:—

Reduction by protochlorid of tin.—Alcoholic solution of protochlorid of tin added to nitrite of ethyl causes violent effervescence, but no disengagement of nitrous fumes. The solution of

protochlorid was added until reaction ceased. The whole was then distilled with caustic potash and the resulting gases conducted into dilute chlorhydric acid, which by evaporation left only a film of substance—this gave on examination distinct indications of ammonia.

Reduction by sulphydric acid.—A large quantity of sulphydric acid was conducted through nitrite of ethyl. Slight effervescence was produced and much sulphur deposited. The liquid filtered from the sulphur proved to contain a large quantity of ammonia, but no ethylamine, di-, or tri-ethylamine, although the mode of examination employed would have detected a very small quantity if present. After the removal of the ammonia, the solution heated with $Pt\,Cl_2$ gave a small quantity of a brown precipitate of a platinum salt, which when heated glowed for a long time showing the presence of much carbon, and finally left a residue of reduced platinum. The amount obtained was too small for investigation, but 20 or 25 milligrammes of Pt. salt were obtained from the decomposition products of 50 grammes of nitrite of ethyl.

This result therefore, as far as to the production of ammonia, corresponds with that obtained by M. Emile Kopp,* although, as that chemist employed sulphydrate of ammonia instead of sulphydric acid, it does not appear that the production of ammonia by the reaction itself, could be positively established.

Reduction by ferrous acetate.—Nitrite of ethyl was added to about three times its bulk of ordinary acetic acid, and sufficient alcohol to make them mix. To this mixture iron filings were added. A powerful action set in without aid of heat. Deutoxyd of nitrogen was evolved in considerable quantity. When action had ceased, the resulting liquid was filtered, evaporated with chlorhydric acid and distilled with caustic potash. The distillate contained only traces of ammonia.

The effect of the reducing agents on nitrite of ethyl is therefore very different. Only with sulphydric acid was any considerable quantity of ammonia produced, and only with ferrous acetate was deutoxyd of nitrogen evolved.

3. *On the Preparation of Urea from Ferrocyanid of Potassium.*

In the preparation of urea by Wöhler's method the amount obtained always falls short of that which should theoretically be produced. Having remarked that even when the operation is

* Gerhardt, *Chimie Org.*, ii, 347.

carefully performed with Liebig's proportions, the lixiviate always contains undecomposed cyanid of potassium, it occurred to me to try the effect of a more thorough oxydation, and the result proved very favorable. The following was the course adopted.

Of roasted ferrocyanid of potassium 850 grammes are mixed with 318 grammes of thoroughly dried carbonate of potash in grains, and fused in an iron vessel. When decomposition is complete, the vessel is a little cooled and 1900 grammes of red lead are to be added, not all at once, but 300 to 400 grammes at a time, with intervals of 10 minutes, stirring and keeping up heat enough to retain the whole in a state of fusion.

After the last addition of red lead the mixture is suffered to remain half an hour on the fire to complete the reaction. Heat is applied in all for about four hours. In this way the cyanid of potassium becomes thoroughly oxydized. The process is concluded in the usual manner, and 500 grammes of urea are obtained. No particular precaution is necessary during the fusion, or even the lixiviation with cold water, but in evaporating the solutions the greatest care must be taken to carry off the vapors.*

Philadelphia, April, 1861.

ART. XX.—*Contributions to the History of Picric Acid;* by M. CAREY LEA, Philadelphia.

Solubility in Sulphuric Acid.

IT is stated in the text-books that picric acid is insoluble in sulphuric acid. It is however soluble to a small degree in strong sulphuric acid; in a more dilute acid it is apparently wholly insoluble, until the dilution reaches a certain point when it increases again. If picric acid be left in contact with oil of vitriol, and the latter be decanted, and mixed with two or three times its volume of water, the picric acid is deposited on cooling in what appear to be very minute square or nearly square scales.

Picric acid crystallizes in the rhombic system, and if we suppose these scales to be formed by predominating $\infty \bar{P} \infty$ planes bounded at the edges by octahedral planes, they should be rhombs approaching very nearly to squares, having their axes as ·9374 : 1·0000.

If cold saturated aqueous solution of picric acid be mixed with sulphuric acid diluted with an equal volume of water, the following results are obtained:

1 vol. solution of picrate, 4 vols. dilute sulphuric acid. (1 vol. acid, 2 vols. water.)	No precipitate, solution remaining as colorless as water.

* For continuation of Mr. Lea's "Contributions," see p. 210.

1 vol. sol. picric acid, 2 dilute sulphuric acid, same dilution.	No precipitate, solution very nearly colorless, faintest tinge of yellow only visible.
2 vols. solution picric acid, 1 vol. sulphuric acid, same dilution.	Nearly the whole of the picric acid was precipitated.

The amount remaining in solution continued in further trials to diminish as the sulphuric acid became more dilute, until a maximum was reached with

3 vols. solution picric acid, 1 vol. dilute sulphuric acid, same dilution.	

It thus appears that mixtures of sulphuric acid and water reach their minimum of solvent power for picric acid when the mixture consists of about 1 vol. acid to 11 vols. water. The proportion of water may be still further increased without materially increasing the solvent power for picric acid. If a cold saturated solution of picric acid be mixed with even but $\frac{1}{11}$ of its volume of sulphuric acid, almost the whole is thrown down.

The fact that the characteristic color of picric acid, which it maintains so persistently through all its combinations, and which is so powerful that, as I have found by actual experiment, a milligramm will distinctly tinge a kilogramm of water, or in other words, that water is colored by one millionth of its weight of picric acid—the fact that this color is totally destroyed by sulphuric acid of a certain strength, without in any way decomposing the acid, is very remarkable. Four volumes of sulphuric acid diluted with five volumes of water exhibit this property, and picric acid dissolves in such a mixture to a colorless solution. This peculiar property has no doubt led to the supposition of the insolubility of picric acid in sulphuric acid above referred to.

Water containing $\frac{1}{10.000}$ of picric acid exhibits a bright yellow color. With $\frac{1}{30.000}$ the color is still distinct, even in a stratum of not over an inch in thickness. But in large quantities a millionth gives a distinct color as above mentioned.

Tests for Picric Acid.

The best tests for picric acid are

Ammoniacal solution of sulphate of copper, which gives a greenish crystalline precipitate.

Alkaline sulphid with excess of alkali, which with heat gives a deep red liquid.

Alkaline cyanid with ammonia, which when heated gives also a red liquid.

The following table will exhibit the relative sensibility of these reagents:

Strength of aqueous solution of picric acid.	Ammonio sulphate of copper.	Potash liver of sulphur (heat.)	Cyanid of potassium.
$\frac{1}{1000}$	Immediate precipitate.	Solution becomes sherry wine red.	Pale sherry red, with heat becoming deep red.
$\frac{1}{4000}$	No precipitate at first, but by standing a few minutes a distinct one.	Deep yellow with a tinge of sherry color.	Deep yellow, tinge of sherry color, deepened by heating.
$\frac{1}{5000}$	Distinct precip. by standing.		
$\frac{1}{10000}$	No precipitate.	The yellow color was slightly deepened, the cyanid test is the more delicate of the two.	

Purification of Picric Acid.

Since my former observations on the purification of picric acid,* I have had occasion to prepare considerable quantities of the acid for my examinations and find that all purifications by converting into potash salt are inapplicable except for very small quantities. The picrate of potash crystallizes out by so small a fall of temperature that the filters, even when kept heated by a double funnel become immediately clogged, and the operation becomes to the last degree tedious and troublesome. As the picrate of lime is very soluble, it seemed probable that it might afford a convenient means of solution; it has indeed been already recommended for that purpose.† But I find it wholly inadmissible. A basic salt is formed which falls to the bottom with the excess of hydrate of lime, and great waste ensues. The insolubility of alkaline picrates in cold alkaline solutions which I have described in a previous number of this Journal, furnished me with an excellent process. The crude acid is saturated with carbonate of soda, an excess of which is to be avoided as it tends to dissolve resinous matter. The hot solution is then easily filtered, and into the filtrate a few clear crystals of carbonate of soda are placed. On cooling, the picrate of soda crystallizes out almost as completely as the potash salt would have done, and all the wearisome delay in filtration is avoided. From the mother water more picric acid may be recovered by the addition of a little carbonate of potash. In decomposing alkaline picrates to separate the acid sulphuric, (and not as usually recommended, chlorhydric) acid should be used, because a moderate excess of sulphuric acid throws down a great portion of the acid which would otherwise

* This Journal, Nov., 1858. † See Gmelin, Eng. Ed., vol. xi, p. 214.

remain in the mother water. A moderate but decided excess of acid is absolutely necessary, because otherwise a portion of alcaline picrate escapes decomposition. Even then, it is advisable to recrystallize the acid from alcohol. If picric acid be dissolved by the aid of heat in a solution of sulphate, nitrate, or almost any salt of potash, more or less picrate of potash will crystallize out on cooling. I have thought this process not devoid of interest, because picric must become more extensively known in the laboratory and in the arts than hitherto.

Effect of Reducing Agents.

The effects of reducing agents when alkali is not present, or not present in excess, (in presence of excess of alkali, picramic acid is formed), are very variable, depending upon slight differences which it is very difficult to seize. I subjoin some of the best marked results obtained.

A mixture of picric acid, alcohol, iron filings, and acetic acid, were digested for an hour at a heat a little below 212°. The filtrate was intensely blue, by standing for half an hour or less, became brown and muddy, depositing a blackish powder, in small quantity, and without trace of crystallization. This filtrate was not changed in color by acids, or apparently affected by them. Alkalies decolorized it. Its shade of blue varied considerably in different experiments, sometimes full blue, sometimes violet, sometimes greenish.

Other experiments were made by acting on picric acid by zinc and dilute sulphuric acid. After an action of some hours, the solution was mixed with alcohol and filtered. The filtrate heated with bicarbonate of potash in successive portions, gave a fine violet liquid, which with further addition of alkali became deep blue with a tinge of violet. According as acid or alkali were present in excess there was more of the violet or blue shade. The colors were always very fugitive, and changed to dirty brown by standing, with deposit of an amorphous blackish powder, (very small in quantity compared with the picric acid used,) which was soluble in acids, and insoluble in alcalies.

These experiments although many times repeated did not lead to the isolation of any substance of interest. There is a certain amount of resemblance between these reactions, and those of some of the decomposition products of phenylamine: the latter contains the radical $C_{12}H_5$ which exists in a substituted form in picric acid.

Philadelphia, April, 1861.

ART. XXI.—*On the Relation between our Perception of Distance and Color;* by Prof. O. N. ROOD.

THE fact that a landscape appears more vivid in color, when viewed by the eyes brought into an abnormal position, as in looking under the arm, &c., is well known.

Some persons have attempted to explain this fact by the influence of an augmented pressure of the blood upon the retina.

In an easy reclining posture, where such pressure can hardly exist, I observe this heightening of tints with great distinctness, also by viewing the inverted image of the landscape by total reflexion through a rectangular prism, the head being in its natural position.

Dr. A. Müller* with more probability has referred this appearance to the different accommodation of the eye for horizontal and vertical lines.

To me it seems that this effect is intimately connected with our perception or non-perception of distance. In gazing at landscapes, the ordinary habit of most persons, artists excepted, leads them to pay attention to the forms and *distances*, (which alone have a practical value as objects of observation), and to neglect the *color*, particularly those portions of it which are subdued. When now by any means the mind is prevented from dwelling on distance, it is thrown back on the remaining element, color; and the landscape appears like a mass of beautiful patches of color heaped upon each other, and situated more or less in a vertical plane.

(1.) A perpendicular position of the eyes reduces very considerably our perception of depth or distance, so that false estimates of it are formed by the eyes in this new situation. With the exception of objects in the foreground, all things seem to lie not far removed from the same vertical plane.

The reason is partly to be found in the fact, that while in normal vision our binocular perception of depth is obtained by regarding vertical lines, trees, &c., in vertical vision the same objects, though instinctively sought afford us no information.

(2.) In normal vision with a single eye, there is certainly, in a binocular sense no perception of depth, nevertheless the mind occupies itself with the idea of distance, and if the objects are familiar there is no augmentation of color perceived. By inverting the image of the landscape with a rectangular prism the objects fall into almost one plane, are diminished in apparent magnitude, and the mind unable to trace distances through this maze, is forced to dwell on the mass of tints presented.

* Pogg., vol. lxxxvi, p. 147.

(3.) With the erecting or inverting telescope, in proportion as the objects viewed are divested of the idea of solidity or depth, can their more delicate tints be perceived. Objects, which in normal vision seem to us nearly without color, are best fitted for these observations; a bare pile of stones and dry mud viewed through a telescope appears often like a richly tinted water color drawing.

It would seem probable that if we could add to paintings of landscapes the element of distance, the mind occupied with this would no longer dwell on the richness of the tints. In confirmation, I find that colored stereographs of landscapes, which out of the stereoscope seem exaggerated in tint, when placed in the instrument no longer appear too highly colored.

From the foregoing considerations, then, it would appear that when the mind is engaged with the perception of distance, the *presence* of color is often overlooked; its *absence* may remain unnoticed from the same cause, for in uncolored stereographs of objects that are perfectly familiar to the observer, it will sometimes be noticed, that those articles which do not greatly differ in color from the tint of the photographic paper, are seen in the stereoscope with an approximation to their natural hues; upon withdrawing the slide from the instrument no trace of such tint is perceived. Objects that are free from lustre, as well known carpets, answer for this purpose. That this should be the case with the tinted photographic representations of white objects can be explained of course in another way.

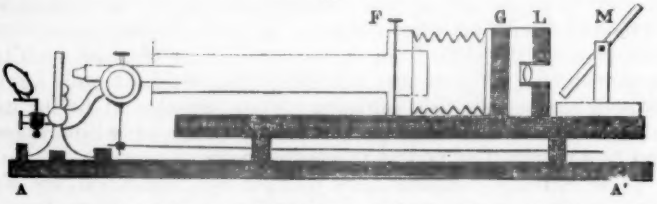
[The chromatic effects here noticed by Prof. Rood are well seen in the ordinary camera and dark chamber. Thus the human countenance when not florid, presents to the unartistic eye few or no traces of pink or flesh color—but every one who has seen it in the camera must have observed with what distinctness the image is colored. The same is true of familiar landscapes, when seen inverted upon the screen in a dark chamber. Here the neutral tints which in nature are almost unnoticed by the common observer, stand out as distinct patches of color in the way so well described by Prof. Rood.—S.]

ART. XXII.—*On the Practical Application of Photography to the Microscope;* by Prof. O. N. ROOD, of Troy, N. Y.

WHILE the value of the photographic delineation of microscopic objects, as a means of accurately recording observations, seems to be generally acknowledged, yet owing to the real or imaginary difficulties with which the process is beset, but very few working microscopists have adopted it.* After eight months of steady experimental work on the subject, this fact appears to me a matter of astonishment, for the difficulties which are not inherent, mostly disappear when proper precautions are taken. I propose to mention briefly certain points in my experience, and to indicate the methods pursued.

Arrangement of the Apparatus.—The microscope is brought into a horizontal position, and connected with a camera box by a blackened paste board tube; much vexation will be avoided by constructing at the outset the arrangement seen in the wood

1.



cut, fig. 1. Blocks are fitted around the foot of the microscope, that it may be firmly held in position, and the camera box slides between parallel strips of board, so that its distance from the microscope can be varied. The length of A A′ is seven feet. The frame holding the ground glass slides in at G; behind it at L, is a door on hinges carrying an achromatic lens of two inches focal length, for the purpose of magnifying the image on the ground glass while focusing; the glass plate should be finely ground. A tube lined with black velvet is to be inserted

* In Vienna microscopic photographs have been produced under the direction of AUER. POHL and WESELSKY have also worked at this subject. (*Sitzungsbericht d. Kais. Akad*, 1857, xxiii, vol. 1, page 317); at an earlier date MAYER of Frankfort obtained fine photographs of this kind. BERTSCH presented similar results to the French Academy (*Comptes Rendus*, 1857, xliv). NACHET also obtained good results. HODGSON (*Quart Journ. of Micros. Science*, 1853, ii, p. 147), DELVES, (3d No. of the same, p. 57), SHADBOLT (ibid, p. 165), HUXLEY (ibid, p. 178 also No. 4, p. 305), WHENAM (same Journal, 1855, No. 10, p. 1), and KINGSLEY (*Phil. Mag.*, 1853, June, p. 461), have published accounts of their more or less successful results. HARTING on the Microscope, *Braunschweig*, 1859. To the above must be added the great work now being issued in numbers in Munich entitled *Atlas der allgeminen theirischen Gewebelehre, herausgegeben von* TH. V. HESSLING *und* J. KOHLMANN, *nach der Natur photographirt von* JOS. ALBERT.

in the compound body, as recommended by Shadbolt, if the eye-piece is not employed. Precautions must of course be taken that light does not enter at unguarded points. At F is a rod connected with a flap of blackened sheet brass in the interior of the box, with which the exposure of the sensitive plate is very conveniently effected. It is obvious that while the operator is manipulating the mirror, or using the stage movements, on account of the length of the apparatus it is impossible for him to see the ground glass, or even to know when light has been thrown on it; a plane mirror, mounted as seen at M, reflects the image of the ground glass, enabling him not only to arrange the illumination with nicety, but to select the microscopic object, and to focus on it approximately. While the mirror is in use the door carrying the achromatic lens stands open; the mirror is afterwards removed and the focal adjustment completed with the help of the lens, by the rod and lever attached to the rack work of the microscope. If the rack work is moderately good, this arrangement is very delicate. When a high magnifying power is employed, it is essential that the microscope be provided with stage-movements to bring the object into its proper position. The lever stage is not to be recommended for this purpose.

Illumination of the object.—That direct sunlight is greatly to be preferred is admitted by those who have experimented on this point. With light from a white cloud I have obtained negatives, in from one to three minutes, with 1 inch and $\frac{1}{3}$ inch objectives; though not highly magnified, they were inferior to those taken with sunlight. Shadbolt obtained negatives by concentrating the light of a small camphene lamp on the object with two lenses. Whenam in repeating this experiment met with no success. I concentrated the light of two flames of a "burning fluid" lamp with a bulls-eye-condenser on the object, employing the 1 inch objective without an eye-piece, and obtained, with several samples of collodion from different manufactures, absolutely no image at all, after an exposure of five minutes. With samples prepared by myself tolerably intense negatives were obtained in four minutes. With the $\frac{1}{3}$ inch objective a faint image was obtained in the same time. For my own work direct sunlight is always employed.

It is well known that the proper display of microscopic objects, requires that great attention should be given to their illumination, not only in degree, but in kind; much has been written on this subject and an astonishing amount of labor bestowed on it. All this applies with double force to the illumination preparatory to the introduction of the sensitive plate; refined methods here find a most useful application. If the power employed be under 100 diam., the plane or concave mirror will answer, if the

stage be provided with a diaphragm-plate having apertures of different size. The mirror should be most carefully adjusted, so that the maximum of distinctness in the image on the ground glass is obtained. I was kindly furnished by Prof. Chas. A. Joy with a *silvered* mirror which Liebig presented to him some months ago while on a visit in Europe. It furnished brighter light than the ordinary amalgam mirror; the use of Liebig's mirrors for this purpose, as well as for ordinary microscopic work is to be recommended. With powers from 100 to 2000 diameters, a condenser of some form is needed. For powers from 100 to 400 diameters, an achromatic condenser adjustable by rack work was used. Such a condenser must be provided with a series of diaphragms having circular apertures differing in size, also with a set of central stops for annular oblique illumination.

Trial alone will settle which aperture gives the clearest image in any particular case. As the lenses of this condenser were not large, I constructed for powers from 400 to 2000 diameters a Wollaston doublet, with an angular aperture of 44°, the lenses being ·5 and ·6 of an inch in diameter. This condenser when provided with a similar set of diaphragms was found to answer very well, both as to the degree and quality of the light, negatives being obtained in 15 seconds enlarged 1500 diameters. As the chromatic aberration was not corrected, it was found easy to illuminate the object either with white or bluish-white light, use of the red or yellow rays being of course carefully avoided.

The proper distance of the condenser from the object is a point of much importance, and is best ascertained by carefully repeated trials. To obtain really good results much nicety in arranging the illumination is required: this is a matter in which microscopists are well practiced, but to secure the best results possible *under the circumstances*, as in photographing test objects, the art and patience of the operator is taxed to the utmost, and several days are often consumed before a really satisfactory result is attained, even in the case of a single object.

Focal adjustment, &c.—Trouble will be saved by selecting the exact object which is to be photographed, by the microscope in an upright position, the instrument is then inclined and connected with the camera. After the compound body is thus placed, if the objective is provided with a "screw collar" for correction, this adjustment must be *carefully made.* Even when this point has received attention, it by no means follows that the chemical focus coincides with the visual, and the exact correction necessitated by this difference must be ascertained by trial. This can be effected by the use of the fine adjustment, (see Shadbolt's paper). Contrary to some other observers, I have found it necessary when using sunlight, with both high and low powers, with, and without eye-pieces, to make this correction

carefully. The use of the rod and the lever and achromatic lens has already been mentioned. After the corrected image has been thrown on the ground glass it will remain nearly unaltered from 30 seconds to 10 minutes, according to the power and mode of illumination employed.

Collodion.—This article when furnished by makers of repute can of course be used, though it is better for more than one reason to be independent of the dealers if possible. A considerable number of samples of pyroxyline were prepared according to different receipts and sensitized variously. The very simple process described by Waldack, on page 266 of his treatise on Photography, was found with slight modifications to yield an excellent article.* The strength of the sulphuric acid was slightly greater than recommended by him, no water was added, the temperature also was slightly higher, at the time of the immersion of the cotton: a more prolonged washing than that prescribed in this work is desirable. This collodion can be sensitized with advantage by the iodid and bromid of cadmium in the proportion of four to one. A receipt published in Humphrey's Photographic Journal has lately been used by me with very good results.†

No. 1.	No. 2.
Plain collodion, 1 oz.	Plain collodion, 1 oz.
Iodid of ammonium, 5 grains.	Iodid of potassium, 5 gr.
Bromid of potassium, 3 do.	Bromid of ammonium, 3 gr.

Dissolve the iodid of ammonium and bromid of ammonium in alcohol, the iodid of potassium and bromid of potassium in the least possible quantity of water, before adding them to the plain collodion. Mix Nos. 1 and 2 in equal parts for use.

This collodion when used according to the wet process, though not very intense when first made, is quite sensitive, negatives of landscapes being obtained in $\frac{1}{4}$ of a second indicating by their strength that a shorter time would suffice. It acquires intensity by keeping. The exposure is effected by the flap at F, and will last from $\frac{1}{4}$ of a second to 4 minutes according to circumstances. The development is as usual, hyposulphite of soda being used as a fixing agent. The use of the bromid of arsenic, mentioned in the same Journal, gave with some samples of collodion, excellent results so far as intensity was concerned.

The negatives thus obtained, are examined by a lens of one inch focal length, to test their degree of sharpness. This quality will not only vary with the manipulation, but with the nature of the object; the sharpest negatives obtained by me, when examined by a power of 40 diameters, appear as well defined as finely executed lithographs seen by the naked eye, while other classes of objects, (dots in pine wood, &c.,) with all care, yield

* Treatise on Photography by Chas. Waldack, Cincinnati, 1860.
† Humphrey's Journal, Joseph H. Ladd, New York.

negatives which present the same appearance under a much lower magnifying power.

Positive prints.—In order to preserve the fine details, the prints should be taken on glass, not on paper; mica answers when a print is to be transmitted by mail. Great care should be used that little or none of the fine markings on the negative are lost in this process; a bright light (sunlight thrown on the negative backed by ground glass,) a small diaphragm before the copying lens, and careful allowance for the chemical focus, are the essentials. To produce enlarged positive prints on glass, the negative is placed on the stage of the microscope and treated like a microscopic object, the magnifying power varying from 5 to 20 diameters. If the prints are to be on *paper,* it will be found that a more liberal use of nitrate of silver and chlorid of gold, than is generally recommended makes success easy.

Magnifying powers employed.—To produce enlarged images the objectives as is well known may be used alone or in connexion with an eye-piece. In the former case with proper illumination, sharp images are produced when the distance between the object (on the stage,) and the ground glass is as great as five feet. With this distance the

1	inch	enlarges	65	diameters.
$\frac{1}{3}$	"	"	190	"
$\frac{1}{7}$	"	"	460	"

In using the objectives in this way the screw collar is set *after* the microscope is connected with the camera.

For more highly enlarged images it is best to add the long eye-piece as has been practiced by some experimenters. The adjustment of the screw collar can then be very nearly completed before the microscope is connected with the camera, which is a great saving of time, it will of course fall nearer the mark "uncovered" than in the first case. However perfectly this operation may be performed in either instance, allowance must still be made for the actinic focus. By varying the distance between the eye-piece and the ground glass different degrees of enlargement are obtained. When the long, or two inch eye-piece, is used, the distance from the object-slide to the eye *lens* being 12 inches, from the latter to the ground glass 34 inches, then

1	inch	enlarges	160	diameters.
$\frac{1}{3}$	"	"	550	"
$\frac{1}{7}$	"	"	1300	"

Powers obtained in this way with the two latter objectives have been used by me with advantage.

Thus with the $\frac{1}{7}$th 113° aperture, the Wollaston doublet of 44° aperture having a central stop, being used as a condenser, I obtained sharp negatives of the *P. angulatum* magnified

1300 diameters with well defined hexagonal markings similar to those obtained by Whenam with a $\frac{1}{12}$th of 130° aperture. Portions of the negative bore a photographic enlargement of 10 diameters. Mr. Whenam announces* that he has discovered by the use of a $\frac{1}{25}$th of large aperture, made by himself, that the markings on this object and on some others, are really due to spherical particles of quartz which can be made by illumination to appear hexagonal. With a power too low, I obtained photographs of the *P. Balticum* with *hexagonal* markings; with a higher power and larger angle of aperture the tendency was to the spherical form.

Photographs by Polarized light.—A Nicol's prism is placed under the stage, one also directly behind the objective, sunlight is reflected from the mirror, and one of the prisms revolved till the field is dark; with the low powers by this simple arrangement photographs of objects may be obtained, which exhibit the structure revealed by the polarized light. For higher powers it is necessary to use the polarizing arrangement described by von MOHL, *Pogg.*, vol. cviii, p. 178, and recommended by Carpenter; that is, the light from a large Nicol's prism is concentrated on the object by an achromatic condenser. The perfection with which this apparatus operates may be inferred, when I state that photographs of the cross and rings in starch granules as well as of the *P. angulatum* in a dark field, were obtained by me without difficulty. Von Mohl remarks, that with inferior apparatus some very distinguished observers have been unable even *to see* these appearances. The selenite stage can of course be used when it is found desirable.

By arranging the apparatus according to the plan adopted by Prof. v. Kobell in his micro-stauroscope, (this Journal, [2], vol. xix, p. 425), the peculiar effects which microscopic crystals produce on the cross and rings of calc spar, can be photographed. By removing the condenser and objective, as well as the slide containing the crystals, beautiful photographs can be obtained of the normal cross and rings; the systems of rings in other crystals can be photographed by substituting them in place of the calc spar, as well as the changes which they undergo by combination with plates of doubly refracting substances, (circular analysis, &c.,) it being merely necessary to introduce the plates or films at the proper positions. I was shown by Prof. Dove some years ago while in Berlin, photographs of the normal cross and rings around the axis of the calc spar, but so far as I know this is the first attempt to photograph the changes which the cross and rings undergo by the action of microscopic crystals.

Stereoscopic photographs of microscopic objects, can be obtained with the monocular microscope, by covering first the right half of

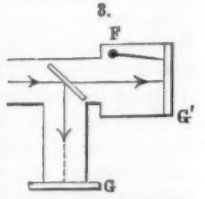
* Quart. Journal of Mic. Science, No. xxxi, p. 145.

the objective, then the left by a suitable brass cap, and taking two successive pictures. When using this method it becomes necessary to move the mirror towards the right or left hand with each successive exposure, which is not only inconvenient, but often produces a slight distortion, that prevents the proper stereoscopic union of the two photographs. On this account I have generally adopted a different plan; the object is placed on an extra stage, which can be inclined from 5° to 10° as seen in profile in the wood cut, fig. 2; it is photographed first at one angle then at the other. In practice the manipulation is easy, and no particular difficulty is experienced from the fact that the extreme right and left hand portions of the field are thrown slightly out of focus. High and low powers can be used equally well. The second negative should be taken immediately after the first, before the illumination has altered. I do not know that stereographs of microscopic objects have actually been taken by other experimenters, though this may easily be the case.

2.

Living organisms offer the photographer some difficulties by their constant motion about the field, and in and out of the focus. It becomes necessary to adopt a plan by which the image can be thrown on the sensitive plate, the very instant after the animalcule has been brought into focus. The following method has been used by me with success to obviate this particular difficulty: a plate of glass with parallel sides is introduced at an angle of 45° into the tube outside of the camera; it reflects an image of the object to the ground glass at G, fig. 3, which is placed so that an equally sharp image of the same object is formed at G′. The sensitive plate is introduced at G′, the flap at F being closed; with one hand the operator, by the aid of the image at G, focuses on the animalcule, just as this is effected, the plate is exposed by the other hand turning the flap. If the collodion is sensitive, a second, or less, suffices to give an image; if a longer exposure be desired the image of the animalcule on the ground glass at G can be watched, and the exposure prolonged till the creature begins to change its position. The real difficulty, in the case of living organisms, is found in the fact that all parts of them do not lie in the same focus; this in fact is one of the most important difficulties connected with the whole subject of microscopic photography. But the introduction of a slight modification in the ordinary compressorium, removes it in many cases; the plate of glass on which the objects rest, instead of being plane, is made slightly *convex*, by the use of a spectacle lens of rather long focus; objects to be examined are placed near the point of contact, and pressure applied as usual, when they are brought nearly into the same plane.

3.



Photographs of opaque objects were obtained by concentrating sunlight on the object, either with the concave mirror properly mounted, or with the plane mirror and bulls-eye condenser. The $1\frac{1}{2}$, 1 and $\frac{1}{3}$ inch objectives were employed. The color of microscopic injections for this purpose should be blue or white, though with a long exposure photographs were obtained of yellow injections.

I am indebted to Dr. Wolcott Gibbs and to G. T. Strong, Esq., for valuable suggestions relating to the subject of this article.

As some interest has lately been manifested with regard to eye-pieces, it may be proper to state, that, in the course of this investigation, three eye-pieces were constructed, No. 1, on the general Huyghenian plan, the eye lens being an under-corrected *achromatic*;* the distance between the eye and field lens could be varied at pleasure, as advised by Amici; as a single microscope its power was eight diameters. The general performance of this eye-piece seemed to me somewhat better than that of the plain Huyghenian eye-pieces with which it was compared.

No. 2 was a Kellner eye-piece, the distance between the lenses could be varied; alone it enlarged 12 diameters. The performance was good. No. 3, consisted of two under-corrected achromatic lenses combined in the Huyghenian manner; alone it magnified 25 diameters. When used as the eye-piece of a telescope it gave a pretty good image; as a microscopic eye-piece, it was inferior to No. 2 in spite of its superior magnifying power, except perhaps when used with well corrected objectives of large angular aperture. With all three eye pieces the correction of the objectives remained unaltered in kind.†

Troy, N. Y., July, 1st, 1861.

ART. XXIII.—*On some questions concerning the Coal Formations of North America.* (Continued from p. 25 of this vol.)

By LEO LESQUEREUX.

Families, Genera and Species of Fossil Coal Plants in the United States.

This general examination is made for a two-fold purpose. First; to see how the remains of fossil plants found in our American coal-measures agree in characters with those of Europe: or rather to find if these remains of ours cannot give some farther light concerning species, genera and families of coal plants, al-

* The orthoscopic eye-piece of Grunow consists of an eye lens partially achromatized in combination with a field lens differing in form from the ordinary Huyghenian.—O. N. R.

† After this article was in print, I received from a friend a paper of AMASA M. EATON, Esq. (*Proceedings B. S. N. H.*, vol, viii, p. 105.) Mr. Eaton has devoted himself with success to the production of *ambrotypes* of microscopic objects.

Troy, N. Y., July 23d, 1861. O. N. R.

ready known from European researches. The fossil flora is very similar to the history of olden times. It is known only by fragments and these must be connected by links and repeatedly elucidated by corresponding data, collected from the monuments of various nations.

Second; to present a comprehensive review of the main facts known up to the present time about our coal flora and to establish, by a critical comparison, the essential characters of the families, genera, and even some species of our coal plants.

§ 1. *Fucoides*, Brgt.

Under this name, a number of vegetable remains of the coal have been formerly described and referred to marine plants. Indeed some geologists have applied the term *Fucoides* to every one of those uncertain forms, mostly stems and roots, which could not be referred to some species known and published before. The *Fucoides* are not only very rare in the true coal measures, but I even doubt if a single specimen of a true marine plant has ever been found in these measures. At least I have seen none, and all the numerous specimens sent to me under that name were incomplete fragments, either of stems or of roots, of some true coal plants.* Mr. Brongniart had already remarked that all the vegetable filaments considered as *Confervites*, like those published by Artis under the name of *Hydatica* and *Myriophyllites*, were roots. Mr. Unger in his classification of fossil plants according to geological formations, (*Genera and species*, page 533), quotes among *Fucoides* of the coal: 1st. *Chondrites Prestvici* (Mor. cat.) an undescribed and unknown species, whose locality is not even mentioned. 2d. *Chondrites dissimilis* (Eichw.) from the mountains of Donetzky in Russia. It is published by Eichwald in the Urw. Russia, p. 89, t. 3, fig. 3, but rather resembles a *Hymenophyllites* than a Fucoid. 3. *Chondrites trichomanoides* Göpp., *Systema foss.*, t. 30, fig. 26, a plant formerly described by the author as a *Trichomanites* and which appears to belong to that section of Hymenophyllites which I have called *Pachyphyllum* (Penn. Report). This plant was found in a kind of shale called by Mr. Göppert *stinkende* with remains of fishes. 4 and 5. *Fucoides Alleghaniensis* (Harl.) and *Fucoides Brongniarti* (Harl.), both belonging to the Silurian or Medina sandstone of our country. 6. *Rhodomelites bijugus* (Eichw.) from the mountains of Russia with *Fucoides subtilis* and *Fucoides tæniola* of the same author. These two last species are not described. Though Eichwald is a very good observer, we can but suppose: either that his four species of fucoides are fragments of coal plants, or that the for-

* We cannot call them either marine or fresh water plants. The coal plants as remarked before, had a peculiar nature, like those of the peat bogs and were appropriated to the formation of the coal. They could vegetate with slight modification of forms either in or out of water and probably both in marine and fresh water.

mation where they have been found is not within the true coal formations. In his *Tableau des Genres*, Mr. Brongniart mentions as pertaining to the *Carboniferous period* two *Chondrites* and two *Amansites*. These last two are *A. serra* and *A. dentata* belonging to the Silurian of Canada; the two others also pertain to the transition formations inferior to the Devonian.

I could easily refer to *Fucoides* a number of specimens undeterminable, branches or stems that look like marine plants. I have especially a smooth, flexuous, linear, apparently rounded branch about the fifth of an inch broad, perfectly equal in its whole length, resembling a piece of *Corda filum* or of *Zostera marina*, found in the shales of the upper strata of the Kentucky coal measures with remains of fishes and of marine shells. But describing such remains and referring them to marine plants would be only an hazardous hypothesis. The stems and leaves of some plants of the coal of Pomeroy are covered with a brownish, thick epidermis resembling the thick leaves of some *Fucaceæ*. But this kind of skin is mixed with remains of ferns and cannot be referred to *Fucoides* without a better proof than this leathery appearance. Prof. Geinitz does not mention any *Fucoides* in his fossil flora of Saxony.

2. *Fungineæ* (*Mushrooms*).

As far as the evidence of fossil plants can be trusted, it seems to prove that species of the mushroom family were living during the period of the formation of the coal. It is especially some of those small, mostly round species of the *Hypoxyleæ* tribe, that have been found attached to leaves and stems of fossil ferns. One of them is described by Prof. Göppert (Syst., p. 262, t. 36, fig. 4), with the name of *Excipulites Neesii* on the leaves of *Hymenophyllites Lobelii*. The same is described again by Geinitz (*Verst.* p. 3, pl. 23, fig. 13,) on the leaves of *Sphenopteris tridactilites*. In this last author we have also, belonging to the same tribe, a *Depaxites Rabenhorsti* and especially the *Gyromices Ammonis*, Göpp. This last species is the only one that has been found heretofore in our Coal measures of America and that I have been able to examine. It abounds on the leaves, the stems and even the naked substance of the shales overlying the coal (No. 3) at Colchester, Illinois. I have seen it also on a piece of fossil stem from Carbondale, Penn. (Specimen No. 711 of Amherst College). It was first named by Göppert, then described and figured by Germar (*Verst.* fasc. 8, p. 112, tab. 39, fig. 169) and more recently by Geinitz. It is a small spiral body about the tenth of an inch in diameter, with the spires progressively enlarging and greatly resembling our *Planorbis parvus* (Say) a small shell now living in fresh water under the leaves and on the stems of floating plants. Its outer end, which in the figures of European authors is blunt and obtuse, appears, on our specimens,

abruptly cut and hollow, like the mouth of a thick shelled mollusc. It is finely striated across; and under a strong lens looks like a beautiful small *Ammonites*. From numerous cross sections of good specimens of this small body, I must admit, contrary to the opinions of the learned European authors who have studied it, that it is a true shell, most probably a species of freshwater mollusc. Internally it is hollow, with a hard, thick, parietal substance, generally of lighter color than the shale. It is true that it is often found within the carbonaceous matter of the leaves and of the stems, and thus would appear to have lived within the substance or under the epidermis of the plants like some *Hypoxyleæ*. But at Colchester, at least, it is still oftener found within the naked substance of the shales. Moreover if it was a shell living on the stems and leaves of the coal, it has been of course imbedded by compression as well in the softened woody and carbonaceous matter of the plants as in the soft clay. Germar has already remarked that he found it on shales, without any apparent connection with vegetable substance. But he supposes the possibility of the destruction of all vegetable substance, except that of this small fungus; a supposition which appears somewhat extraordinary. The presence of fresh water shells in the bogs of the coal can not be doubted after the remarkable discovery by Prof. Dawson of a fresh water *Pupa* in the coal fields of Nova Scotia.* And the scarcity of these molluscans in the coal measures is in accordance with what we see on the peat bogs of our time, where the number of fresh water shells is extremely limited. I can not but say again how difficult and hazardous it is to determine such small bodies attached to petrified stems and leaves, from the impossibility of examining their internal structure and of finding the spores. External and variable forms exactly like small fungi are often mere unorganized bodies, produced by some mechanical or chemical action. All the remains of plants and even the shales overlying the semi-anthracite coal of Trevorton, Penn., are covered with small round vesicles, of different sizes, looking exactly like *Spheriæ*, and are filled with a brown powder resembling spores. Sometimes the coal itself is full of them. They have probably been formed by the ebullition of the whole matter accidentally stopped during a strong gaseous emission.

Another very remarkable fungus, *Polyporites Bowmanni*, is described by Lindley and Hutton in the fossil flora of Great Britain, (vol. i, No. 65). This species, or at least one, agreeing exactly with the figure and description of the English authors, was found in black bituminous shales overlying No. 1B coal at Johnstown, Penn. It is an hemispherical or reniform body, marked with concentrical zones, especially distinct near and

* Proceedings of the Geological Society of London, (Dec. 14, 1859).

along the margin. As Prof. Lindley remarked it: "it is like one of those fungi belonging to the genera *Boletus, Polyporus, Thelephora, Dœdalea*, &c., (our species rather resembles a zoned *Boletus*), which attach themselves to their support by one side, projecting forward from it and increasing by periodical additions to their margin, in consequence of which that part assumes a zoned appearance." The lower part of these Fungi, the *Hymenium*, is a more or less thick compound of vertical, cylindrical or angular pipes or tubes closely united together and containing the seeds. It separates more or less easily from the upper part or the hat of the Fungus. Our American specimen is more perfect or better preserved than the two which have been found in England. Two-thirds of the surface show the disposition of the zones of what I consider to be the lower part of the hat or *Pileus*. The balance of the surface, just in the middle of the specimen, is occupied by what appears to be a piece of the undetached *Hymenium*. This part, which is concentrical and exactly in concordance with the zones, is even evidently zoned though less visibly than the *Pileus*, and formed of square, radiate and concentrical areolæ, running around a central point and from it to the margin. This confirmation can not be compared to that of a scale of fish. It is exactly that of a compressed and petrified *Boletus*. The celebrated English author was only prevented from giving the same decided opinion by the mark, on the surface of specimens, of some lines *which are not in accordance with the radial lines near the margin*. This appearance is probably caused by the superposition and agglutination of some piece of another plant. The American specimen has nothing like it and it removes the only objection made against the admission of this species as a true Fungus or mushroom. The black bituminous laminated shales where it was found contain together with it abundant remains of *Lepidodendron*, especially of its leaves and cones.

2. *Lichens, Mosses and Liverworts.*

Brongniart and other palæontologists have already remarked as a peculiar phenomenon, the absence of every trace of fossil Lichens, Mosses and Liverworts in the old formation of our earth. A few mosses and Hepaticæ only have been observed in formations no older than the tertiary,* and especially in pieces of amber in Germany.†

Many years ago, I found around Pottsville, Penn., a kind of laminated soft gray shales, splitting in thin lamellæ and resem-

* Heer's Flora tertiary Helvetica and also Ungers' Genera; Dunker's Monegr.; and Brongniart's fossil flora.

† H. B. Göppert and G. C. Berendt, die in Bernstein befindlichen organischen Reste der Vorwelt.—Göppert in Berichte der Berlinen Academy, 1853.—Menge Beitraege zur Benstein flora in den Schriften der Nat. Gesell., zu Danzig. Bd. 6, H. 1.

bling pasteboard, whose surface bears the traces of the remains of very small, innumerable fragments of thin filaments, mixed with an indistinct compound of what appears to be very small detached oval leaflets. At first, I compared them to, and found them to resemble those indistinct forms of leaves and stems, which are seen on the surface of the pieces of peat, when this matter has been formed by the Sphagnaceæ, a tribe of the Mosses. I have come again and again to the examination of those peculiar shales, expecting to find some distinct outline of the plants which cover them with their remains. But nothing more can be seen than what I had discovered at first, and this is not enough to authorize the conclusion that these remains are those of certain mosses.

4. *Filices*, (*Ferns*).

The following remark of Mr. Brongniart in his *Tableau des genres*, (p. 15) is fully confirmed by the examination of all the specimens of fossil-ferns that I have been able to collect from our coal measures. He says: "*that he is satisfied that in the classification of fossil ferns, we must establish genera from the attentive study of the nervation and of its relation with the general form of the pinnules and of the fronds. That we can not look for reliable characters to the fructification, until it has been possible to observe the sporanges or fruit-dots of the great majority of the species,*" &c. The number of fruiting species of fossil ferns, found in the coal measures of the United States, is already great indeed. But I have not been able to observe in a single case anything else about the fructification, except the position of the spores relatively to the nerves. And this, even, seldom. The true characters used for the classification of the living ferns, viz., the form of the fruit-dots, their mode of attachment and their involucre or *Indusium* could not be ascertained in a single case. Among the most remarkable and distinct fruiting specimens of ferns, I have got two at least, and perhaps three, which show the sporanges separated from the leaves, appearing to be born on separate pedicels, as it happens in many species of our time. One is *Staphylopteris stellata* Lsqx.,* from the sub-conglomerate coal of Arkansas. Another, undescribed represents small bunches of round, flattened sporanges, somewhat inflated on the margins, and attached on a bipinnately divided branch, on both sides of a common narrow rachis, resembling the medial nerve of a *Pecopteris*. The whole bears some resemblance to a fruiting branch of our common *Botrychium Virginicum*. It might be supposed from the dispcsition of the sporanges, which is like that of *Asterocarpus Sternbergii*, Göpp., that the substance of the

* Second vol. of the Geol. report of the State Survey of Arkansas.

leaflets supporting them has been destroyed by maceration; but there is no trace of any such substance and the sporanges are at some place irregularly distributed. A third species, also undescribed, represents a few narrow leaflets, the upper part of a pinna, with very strong, arched, dichotomous, reticulated nerves, marked, in relief on the stone, appearing like the supports of sporanges destroyed by maceration. I consider it as the remains of the fruiting branch of a *Neuropteris*. But all these fruiting branches can not be referred except by mere supposition to species of fossil ferns known by their leaves and classed by their nervation. They must be described separately as fruit and their relation to peculiar species of ferns can not be mentioned, till they are found in connection with their leaves.

Of course this can not change the views expressed above, concerning the classification of the fossil ferns, but only force the admission of one or more of those artificial and unreliable genera, which may be eliminated by the discovery of better specimens and with which fossil botany has to be satisfied for the present.

With the exception of a few species which can be separately classed as species of doubtful affinity, all our fossil ferns may be contained in the three following tribes: 1st, *Neuropterideæ*, 2d, *Pecopterideæ*, 3d, *Sphenopterideæ*. The great Palæontologist Göppert, in reviewing in his *Genera* (liv. 3 and 4, p. 48), a former classification of his *Systema*, has admitted two other tribes, the *Danaeaceæ* and the *Gleicheniaceæ*. But we have not thus far in our coal measures any representative of the first; and our species of the second, like *Gleichenites artemiscæfolius* Göpp., (*Sphenopteris* Brgt.,) belong by their nervation to the *Sphenopterideæ*. Both these divisions may thus be left aside for the present.

1st Tribe. *Neuropterideæ*.

The classification of our American species seems to necessitate a slight modification in the subdivision of this tribe admitted by the European author. I would subdivide it in the following genera: 1st, *Noeggerathia* Sternb., 2d, *Cyclopteris* Brgt., 3d, *Neuropteris* Brgt., 4th, *Odontopteris* Brgt., and 5th, *Dicthyopteris* Gutb.

1st, *Noeggerathia* Sternb. This genus was first established by Sternberg, *Vers.* p. 28 for the description of the figure of its plate 20th; then by a more appropriate description page 33, and definitively page 36th of his 4th book where he limits as follows the genus *of uncertain affinity: Caudex ignotus; rami teretes, pennam anserinam æquantes, lignescentes; folia alterna, approximata, obovata, ramum basi semi-amplexantia, apice pectinato-dentata, integerrima.* The author does not mention the nervation, but his description is completed and somewhat modified by Mr. Göppert

who in his Genera (liv. 5 and 6, p. 107), has described another specimen of Sternberg's species, *Noeggerathia foliosa:* and fixed the genus thus: *Frondes petiolatæ, pinnatæ; pinnæ obovato-cuneiformes vel obovatæ, lateribus petioli applicitis semi-amplexicaules, nervis numerosis teneribus, plerumque simplicibus ab ima basi adscendentibus percursæ.** Göppert then describes two new species of this genus: *N. obliqua* and *N. Beinertiana.* The figure of both represent only part of much longer leaves than those of Sternberg's species. These leaves lacerated on one side have an appearance far different from that of the Ferns, to which nevertheless Mr. Göppert refers this genus. Brongniart, in his *Tableau des genres* has described again this *Noeggerathia foliosa* and from the simple pinnate form of the frond, the rigidity of the leaflets and the mode of nervation, he compares it to a palm or rather to the American *Zamiæ.* He has thus made a separate family of the *Noeggerathiæ*, and places it between the *Cicadeæ* and the *Conifereæ.* This Family contains only two Genera: *Noeggerathia* and *Pychnophyllum*,† this last one replacing the *Flabellaria* of Sternberg or rather including only *Flabellaria borassifolia* Sternb. Mr. Geinitz in the *Versteinerungen*, &c., has apparently admitted Brongniart's views. He places the family of the *Noeggerathiæ* with the Dicotyledonous plants having the same two Genera and refers to *Noeggerathia* a number of fossil fruits of the Genus *Rhabdocarpos.* This variety of opinion and uncertainty of the characters of both the genera *Noeggerathia* and *Cordaites*, is perplexing indeed. But I think that the examination of our American specimens, referred to these Genera may help to fix their true characters and place.

It is evident that both the species published in the report of the State Geological Survey of Pennsylvania as *Noeggerathia minor* and *N. obtusa*, are true *Noeggerathia*, according to the description and the figure of Sternberg's species. They correspond not only with Sternberg's and Göppert's description of the genus, but with the remark of Brongniart about the nervation and the rigidity of the leaves. The branch figured on plate 1 fig. 10, shows that these plants were at least bipinnately divided. Since the publication of this report I have had opportunities to examine better specimens of the same species and especially have lately received from Prof. J. D. Dana of New Haven, the figure of a splendid specimen of *Noeggerathia obtusa*, found in the old Red Sandstone of Montrose, Penn., by Rev.

* The restriction *plerumque simplicibus* can not be admitted for such leaves as those of *Noeggerathia foliosa* when the nerves all come from the base. They must of course much divide in ascending.

† This genus is wrongly ascribed to Göppert by Geinitz in his *Versteinerungen*, &c., p. 40. In the *Tableau des genres*, by Mr. Brongniart (published in 1849), the author establishes the genus *Pychnophyllum* for replacing *Flabellaria.* Mr. Göppert admitted it only in 1852. Brongniart's genus has even the priority over *Cordaites* proposed by Unger in 1850. But this last name should be preserved as a well merited compliment to Corda who has so admirably described and figured the *Flabellaria borassifolia* (Sternb.).

Henry A. Riley. It shows the upper part of a frond with three oblique pinnæ somewhat reflexed from their base and the pinnules or leaves, broadly oval or reniform, the upper one flabellate, all narrowed to the base and pinnately attached on both sides of the rachis by a narrow decurring base. The point of attachment of the leaves is just as I have figured it in my report. This splendid specimen has evidently the general outline and the appearance of a fern and at once puts aside Brongniart's surmise that the simply pinnate form of the leaf, &c., show it to be analogous to the *Zamiæ*.

On the other side, I have been able to find in the Anthracite basin of Pottsville, a stem of *Cordaites* with some of the leaves attached to it. It agrees exactly with the one figured by Corda though less well preserved and entire. It is a simple stem, about half an inch thick, bearing numerous long, ribbon-like, clasping leaves, spirally placed around it, marked with sharp, narrow, parallel, mostly simple nerves. The leaves are scarcely narrowed at the base and thus nearly linear; but, as I have seen it many times and as Corda's figure shows it, the leaves near the top of the stem become shorter somewhat narrowed at the base, spathulate-oblong, just of the same form as both the leaves of *Noeggerathia obliqua* and *Noeggerathia Beinertiana* figured in Göppert's Genera. I will not say that Göppert's species are not true species or belong to the same *Cordaites* as ours; but I believe that all those broken leaves described by Göppert as *Noeggerathia* ought to be referred to the genus *Cordaites* and removed from the Fern-family. I think that the genus *Noeggerathia* should be characterized as follows: *Frond bipinnately divided. Pinnæ, long, linear, oblique, flexuous; pinnules alternately and pinnately placed on both sides of the rachis, enlarged above, obovate, obcordate or reniform triangular, narrowed at the base and obliquely attached to the rachis by a narrow sometimes slightly decurrent base. Nerves equal, numerous, emerging from the base and forking in ascending.* To this genus thus limited I would refer *Noeggerathia foliosa* Sternb., *N. minor* Lsqx., *N. obtusa* Lsqx., *N. flabellata?* Ll. and Hutt., *N. Bockschiana* Lsqx., *Cyclopteris dissecta* Göpp., *C. hybernica, C. McCoyana? C. Jacksoni* Daws., with some other *Cyclopteris* with a narrow angular base and even perhaps *Odontopteris imbricata* Göpp. If we except of these species *Noeggerathia flabellata* and *N. foliosa* both of the lower coal, the first of England, the second of Bohemia, all the other species pertain to the Old Red Sandstone and thus the botanical classification very well agrees with the geological distribution. For this reason, especially, I doubt if *Noeggerathia cuneifolia* Brgt., of the Permian of Russia belongs to this genus, or it may be, as I have remarked it formerly, that, as this species is found with a *Lepidodendron*, its geological horizon has not been exactly marked.

To the genus *Cordaites* thus characterized: *stem simple, annulate or marked by the persistent base of the leaves; leaves simple, clasping at the base, long-linear, marked by simple, equal, parallel, rarely forking nerves:*—I would refer all those ribbon-like leaves so abundant in our coal measures, and generally found in broken fragments. *Noeggerathia palmæformis* Gopp., recently found in the coal fields of Illinois, *N. Beinertiana* Gopp., *N. ovata*, *N. abscissa*, *N. dichotoma*, *N. tenuistriata*, *N. Bruckeriana*, *N. crassa*, all species of the same author are referable to *Cordaites*. At least as much as can be seen from the figures that mostly represent fragments of leaves.*

2. *Cyclopteris* Brgt. By somewhat extending the definition of the genus *Cyclopteris*, it would be easy to place in it most of the species, if not all, of the genus *Noeggerathia*. But the name of this last genus has the priority to that of the former, and moreover the fine descriptive name, *Cyclopteris*, represents a kind of leaves far different from those to which Sternberg applied the name of *Noeggerathia*.† At first, it contained a number of species established for isolated round leaflets which a more careful observation showed to belong to species of *Neuropteris*. As the relation of species of *Cyclopteris* to species of *Neuropteris* is not easily ascertained, Brongniart made for these doubtful forms a new genus with the name of *Nephropteris*. Numerous American specimens have furnished satisfactory evidence of the identity of most of the species of *Nephropteris* with some species of *Neuropteris* and *Odontopteris*. They are mostly large, round leaflets, placed at the base of the primary pinnæ at the point of union with the common rachis or sometimes pinnules or leaves of an abnormal form, placed on the common rachis between the primary pinnæ or even on the pinnæ, between leaves of a normal form. We can thus eliminate the genus *Nephropteris* and refer to *Neuropteris* the species formerly belonging to it. I can scarcely refer any American species to the genus *Cyclopteris*, as it has been limited by Brongniart in his *Tableau des Genres: frond simple, pedicellate, symmetrical, round, cordiform* or *flabellate, entire* or *lobed, without traces of medial nerve, all the nerves coming out from the basis of the leaf and diverging in dividing or forking to the border.* I have found only a small incomplete specimen of *Cyclopteris flabellata* Brgt. It is no longer in my possession and I am unable to say if it represents exactly the European species. I know nothing more of it but what is said on p. 855 of the Report of the Pennsylvania survey.

3. *Neuropteris* Brgt. The celebrated Göppert had in his *Systema* admitted a new genus, *Adiantites* for a number of these

* Prof J. W. Dawson has adopted the same views in a paper recently published in the Canadian Naturalist.

† Jac. Nöggerath to whom the genus is dedicated is author of valuable researches on fossil trees and stems published at Bonn, 1820–21.

species intermediate between *Cyclopteris* and *Neuropteris*. But in his *Genera*, liv. 5 and 6, p. 90, he puts the classification aside and admits Brongniart's genus *Cyclopteris* with some modification, so as to place in it a number of species which I consider true species of *Neuropteris*. Indeed, the essential character separating his genus *Cyclopteris* from *Neuropteris* is, for the former, the nerves emerging from the base of the leaves and flabellately diverging and dichotomous in ascending; while in the genus *Neuropteris*, the medial nerve is marked from base up to above the middle of the leaves and disappears near the summit. Now, it is certain that a number of our *Neuropteris* have for the same species the two kinds of nervation that characterizes Göppert's genera. In *Neuropteris hirsuta* Lsqx., the long leaves are generally strongly nerved up to the middle; while the round leaflets, attached at their base, as also the upper leaves of the penæ, which become simple, have all their nerves flabellate from the base without a trace of a middle nerve. *Neuropteris Clarksoni* Lsqx., bears on the same pinnæ two kinds of leaves, the one longer, narrower and strongly nerved in the middle; the others shorter, broader, with all the nerve flabellate from the base. This *Neuropteris* appears to be the American representative of *Neuropteris auriculata* of Europe, essentially differing from it by the strong medial nerve that marks some of its leaves. In *Neuropteris Loschii* Brgt. so common in our coal-fields, the medial nerve appears only with the larger leaves of the inferior secondary pinnæ. In *Neuropteris Desorii* Lsqx., again, the leaves attached to the primary pinnæ are marked with a strong medial nerve, ascending nearly to the point, while a number of leaves, attached on the primary rachis, have not only all their nerves flabellate from the base, but are enlarged in outline and take the form of *Cyclopteris*. All our species of *Neuropteris* show the same variety of nervation. Accordingly, I think that it would be more convenient and rational at the same time, to fix the genus *Neuropteris* as follows: *Frond pinnately, bi- or tri-pinnately divided; pinnules or leaves of various forms, round, oblong, oval, mostly entire, sometimes lobed, cut or fringed, attached to the rachis only by the middle point of their base. Medial nerve sometimes distinct and vanishing above; secondary nerves numerous, either obliquely emerging from the medial nerve or flabellate from the base, all arched and dichotomous.**

4. *Odontopteris*. The only essential character which separates this genus from the former is that in *Odontopteris* the pinnules or leaves though separated from each other, are attached to the rachis by their whole base or by the greater part of it. The nervation is very variable; the nerves either running from the whole base and straight as in *Odontopteris Schlotheimii*, or emerging

* As the genus is here limited, both the species *Neuropteris Moorii* Lsqx., and *Neuropteris Adiantites* Lsqx., ought to be separated from it and placed elsewhere.

from an enlarged base, flabellate, dichotomous, and arched as in the larger leaves of *Odontopteris Alpina*, Sternb. This last species, a beautiful one, has been lately found in the Anthracite coal measures of Rhode Island,* together with a number of our common coal plants, mostly the species characteristic of No. 3d and No. 4th coal. Our specimens are far more complete than any of those published in Europe. The frond, tripinnately divided has the primary pinnæ very obliquely separating from the common broad, narrowly and equally striated rachis. The secondary pinnæ are alternate and open or perpendicular to the rachis. The leaves or pinnules of the lower pinnæ are distinct, somewhat distant, oblong, obtuse, slightly scythe-shaped outward, one to two inches long, attached to the rachis by two-thirds of the rounded base. In the upper part of the frond, the secondary pinnæ are shorter and their leaves or pinnules are also much shorter, reniform or ovate, sometimes united together by the decurrent base, the terminal pinnule is very long, oval-lanceolate, obtuse. Generally the pinnules are shorter on the upper side of the pinnæ, a character already slightly marked in Sternberg's figure. The leaves of the lower pinnæ have exactly the nervation of the genus *Cyclopteris* of Göppert or of the *Neuropteris* with flabellate nervation. On the upper pinnæ, the leaflets even in the same pinna have either a flabellate nervation from the middle of the base, or more generally the nerves emerge from the rachis along the whole base of the leaflets, thus showing that the species is a true *Odontopteris*, as Geinitz has remarked it.

Large specimens of *Odontopteris Schlotheimii* Brgt., obtained from our coal-measures, show that this species found only in fragments in Europe, is tripinnately divided. The primary pinnæ about one foot long are lanceolate, half open; the secondary ones are linear, pinnately lobed with the lobes round or oval, united together near the base or distinct. In this species as in the upper pinnæ of *Odontopteris Alpina*, the first leaflets near the point of attachment of the rachis are generally distinct and their nerves run out from a point in the middle of the base of the leaflet and not along the whole base as in the other divisions.

Another fine and remarkable species of *Odontopteris* was also found recently in connection with No. 1B at Murphysboro, Ill. It is *Odontopteris heterophylla* Lsqx., (in MSS. report of the Geol. State survey of Illinois, pl. 2, fig. 2 to 5). The frond appears bipinnately divided. Pinnæ lanceolate in outline or cordate-ovate. Pinnules or leaves, either entire, obovate-obtuse decurrent on the rachis, becoming broader, shorter, cuneiform-oval near the base of the pinnæ; or elongated, diversely lobed, with unequal, linear-long, or short-lanceolate-oval divisions; terminal

* I am indebted for the communication of a fine specimen of it to Mr. James H. Clark of Newport, R. I.

pinnule large, deltoid, obtuse, alternately lobed on each side. Nerves sharply marked, inflated near the base two or three times forked; in the decurrent leaflets, the nerves are also a little decurrent along the rachis; in the other leaflets they run out from the whole base.

5. *Dictyopteris* Gutb.—This genus so well characterized by its nervation has still but one representative species, abundantly found in the whole extent of our coalfields, *Dictyopteris obliqua* Bunb. The form of the leaves is variable like their size. The upper pennæ are only pinnately lobed and the lobes separated to the middle, and short and nearly round.

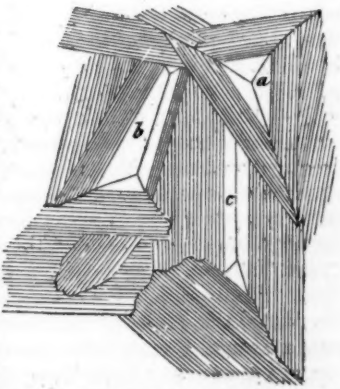
Columbus, Ohio, July 4, 1861.

(*To be continued.*)

ART. XXIV.—*Observations upon the Freezing of Water at the Passaic Chemical Works, Newark, N. J.;* by ARTEMAS BIGELOW.

ONE of the furnaces having been stopped in the early part of the month of Jan., 1860, the cistern for cooling acid before it is bottled, became frozen on its surface about two inches deep. The cistern was within the building and near the side of it. The weather but moderately cold. On the side farthest from the building the water had oozed up through and overspread the surface, slowly freezing at the same time. Attracted by the appearance of this portion of the ice, I examined, and found it composed of plates or laminæ lying one against the other obliquely to the surface of the water. The masses of plates were also at various angles to each other, leaving many open spaces of regular forms, *a*,*b*,*c*, fig. 1, whose faces were beautifully smooth and perfect, meeting below from their obliquity. The origin of each mass of plates being independent of the others, and extending from below obliquely upwards in three or four different directions, a cavity would consequently be left between the divergent masses, (fig. 1). These laminæ were free, except below, so that a knife thrust underneath lifted them out separately. Also as often as the thumb was applied with pressure to their upper edges, the lines would disappear and the ice at the place of pressure look solid,

1.



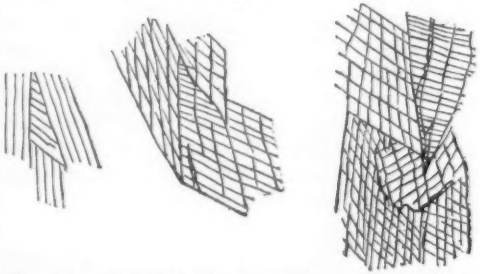
a, *b*, *c*, are cavities, the inclined sides meeting at the bottom.

while moisture oozed out on the surface; but, on the instant of its removal the plates regained their appearance and position; showing that there were intervening spaces filled with water. The extent of surface was about two square feet.

Directing the water to be drawn off, I left it till the next morning, when, the cold having increased, the ice was found solid and transparent, the lines obliterated and the cavities remaining. The next day was warmer, the ice began to melt, and the plates were again apparent, less perfect than before but defined and so loose as to crush under the least pressure. Wherever it was still firm, pieces often a square inch in extent, broken off, frequently showed a brilliant iridescence. The iridescence was in the structure and appeared to be from the surface of a plate not in the same plane with the series adjoining it. When seen edgewise it presented a well defined line like a fine crack, but no separation could be made by a knife any more in that than in any other direction.

A few days after, I found the water in the urn on the top of the office stove frozen except a central cavity. A fire had been made and the ice began to melt. On the top, at the centre, the plates were visible, but imperfect from the melting. Taking out the mass and holding it up to the light, the inner surface was beautifully marked throughout with strong lines at similar angles to each other as existed in the plates above first mentioned, but crossed by finer lines invariably at an acute angle, forming rhombs whose angles were slightly rounded. Fig. 2.

2.



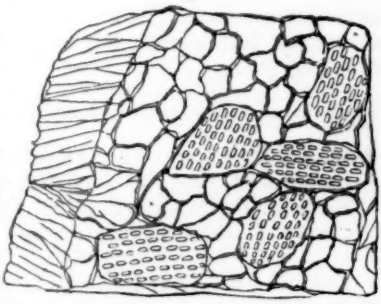
I looked daily for these appearances, as I passed back and forth, for nearly a mile, between the canal and the river, and frequently during the mild weather of the succeeding winter months observed the facts mentioned in the first case, with this difference, that when the cavities were apparent in thin ice formed as usual on the surface of the water, the sides diverged downward, being the reverse, in position, of those mentioned in case first. When water freezes rapidly, the originating centres must be many, the series of plates of small extent, the cavities filled up, a homogeneous mass formed, and no sign of its structure apparent until the slow thawing of spring reveals it, as we shall soon show.

Again, one morning in February, stepping down to the edge of the river I noticed glistening particles of ice upon chips and sticks in great abundance, which proved, on examination with a magnifier, to be hexagonal hopper-formed bodies upon small icy pedestals. The diameter across the top of the hexagon was about one line. The form was very perfect and beautiful, with fine lines running around as if they were the divisions of the crystalline layers, as is shown in the accompanying magnified drawing. Fig. 3.

3.

During the month of February, masses of ice were piled up along the shore above the reach of the tide, and after exposure to the warm sun of that month, lost their vitreous transparency and became nearly snowy white. Their surface was divided up into irregular or oval forms separated by grooves, and lines ran through their thickness, like the joinings of a compact mass of irregular crystals. When a large mass was thrown or let fell upon another mass of ice it broke easily into innumerable crystals. These crystals are irregular and can be called such only to distinguish their general appearance, but almost uniformly show the obliquity mentioned above.

4.



One such mass, fig. 4, had in the large divisions on its surface, oval spots, separated by depressions due to more rapid melting in the joinings. They struck me as parallel to the second case mentioned above, except in this case the angles are completely rounded off by melting.

The noticeable points in these facts seem to be, 1. That water in freezing, under favorable conditions of temperature, forms plates containing between them thin layers of water. Why these water layers do not freeze at such temperature I am unable to tell, unless the air bubbles of the parts crystallized are driven into the intervening spaces and there confined, preventing the water layers from losing heat so rapidly. Whatever be the reason of its higher temperature, the same cause enables those layers when frozen, to melt more easily than the plates, even to the depth of many inches. If the idea be correct that the water layers contain numerous minute air bubbles, then the sun light in passing through the ice, must give up accumulating heat to these bubbles, like the sun shining into an air-tight glass globe. But whatever the cause, this method of freezing and thawing accounts for the manner in which the ice of the great

lakes and other large bodies of water breaks up in the spring. The presumption is that all ice forming on water is produced in the same manner. But at a very low temperature, the crystallizing centres are so many and the freezing of the plates and intervening spaces so rapid, that the process could not be distinguished. Yet all field ice from this latitude to the arctic regions breaks up in the same manner, by first becoming porous or "rotten." I would ask, can the snows of the glaciers when turning to ice, go through such a process of crystallizing, and can it have anything to do with its supposed plasticity?

2d. The uniform obliquity of its plates, the rhombic markings in case 2d, and the oval spots in the last case indicate a rhombohedral form in the ultimate crystals.

Whether observations similar to these have been made by others I do not know, but they interested me and led me to make the accompanying drawings, and also stereoscopic pictures of some ice masses which had lain exposed for many days to the action of warm sun light. I had intended to renew these observations last winter, but circumstances prevented me from doing so.

ART. XXV.—*Note on the probable Age of the White Limestone, at Sussex and Franklin Zinc Mines, New Jersey.* In a letter to Profs. J. D. DANA and BENJ. SILLIMAN, Jr.; by GEORGE H. COOKE, Prof. of Chemistry, &c., New Brunswick, N. J.

Dear Sirs,—At our late visit to Franklin Furnace, Sussex Co., N. J., I had the pleasure of pointing out to you, the faithfulness of Vanuxem and Keating's original description of the geological occurrence of the rocks of that locality; and the correctness of their conclusion, that the white and blue limestones found there, are of different ages. Since their description was written it has been asserted that the white limestone is of the same age as the blue limestone, though changed in appearance by metamorphic action.

Being confident that the facts cited prove the correctness of the former conclusion and the consequent incorrectness of the latter, I quote portions of the paper which bear upon the above points.

The original article is published in the Journal of the Academy of Natural Sciences, Philadelphia, vol. ii, p. 277, and is entitled, "On the Geology and Mineralogy of Franklin, in Sussex Co., N. J., by L. Vanuxem and Wm. H. Keating." They say, "The whole country to a great distance is composed of what may be considered as sienite. . . . The sienite of Franklin is found in beds or layers of variable thickness, running in a direction parallel to that of the ridge from the N.E. to the S.W. . . . The

layers or beds incline to the S.E., dipping under at an angle of about 80°. Subordinate to this sienite are found limestone, gneiss, and greenstone. . . . The limestone forms a bed without any apparent parallel seams or divisions, and is peculiarly characterized by its eminently crystallized structure, consisting of large straight lamellar masses, confusedly aggregated. . . . It is of a fine white color, presenting in some instances, a pearly lustre, slightly chatoyant. . . . The direction, inclination and dip of this limestone are the same as those of the aforementioned sienite.

"Next to the sienite, but evidently of a later formation, is found a mass of grauwacke of no great thickness. This grauwacke is generally fine grained, of a light gray color; the fragments, as well as the cement which connects them, appear to belong to quartz. It is found on the north side of the ridge in thin beds or layers, directed from the N.E. to the S.W. and dipping to the N.W.; this grauwacke is evidently of posterior formation to the sienite, and must have been formed after the surface of this rock had undergone those changes which we at present observe in it; for instead of presenting a parallel stratification, it is found inclining in a diametrically opposite direction, and covering the edges or crest of the layers of sienite. The grauwacke and its mode of superposition can be well observed on the road from Franklin to Dr. Fowler's (house) at about a quarter of a mile below the furnace; it is covered by a blue limestone which rests upon it in parallel superposition; this limestone is found in layers or beds of variable thickness; its color is a pale gray sometimes of a deeper gray passing into blue; its texture is compact or subsaccharoidal; near the grauwacke it is slaty."

A locality, where the meeting of the two rocks is plainly exposed, is on the left of the road passing from the tavern at Franklin Furnace to Col. Fowler's and but a few rods from the former place. The gneiss will be seen dipping at an angle of 75° to the S.E. and the sandstone (grauwacke) which has the lithological appearance of Potsdam sandstone, lying immediately on the upturned edges of the gneiss and dipping at an angle of 45° to the N.W. The strike of the two rocks is different;—the sandstone being 15° or 20° more to the S.E. and N.E. than the gneiss. The sandstone is as near as I can judge from its limited exposure not more than 30 feet thick; the slaty limestone, which lies upon the sandstone, is somewhat thicker; perhaps 70 or 80 feet thick, and then comes a cherty limerock of great thickness. No fossils have been found in this vicinity, either in the limestone or sandstone. Dr. Kitchell, Superintendent of the late Geol. Survey of New Jersey, recognized the unconformability of the gneiss and the blue limestone and in his Annual Report for 1855, p. 133 and onwards, refers to several localities where this can be seen.

New Brunswick, N. J., July 2, 1861.

Art. XXVI—*Chemical Contributions;* by M. C. Lea.

4. *On the Action of Nitric Acid on Picramic Acid.*

On this point very conflicting statements have been made. Girard, and Pugh, respectively state that picric acid is reproduced by the oxydation of picramic acid by nitric acid. A similar statement is made by Kolbe in his Lehrb. d. Org. Chemie (authority not given). In a paper published several years since, on picric acid, I expressed a similar opinion. On the other hand Wöhler stated that his nitrohæmatic acid (now known to be identical with picramic) was not reconverted to picric acid by the agency of nitric acid. Gerhardt too, in quoting the first opinion puts a note of interrogation after it, as if to express a contrary conviction. These differences of opinion have induced me recently to re-examine the subject and have led to the conclusion that the substance formed is not identical with picric acid. The following were the reactions observed.

Picramic acid readily dissolves in strong nitric acid to a dark brown solution. By 15 minutes boiling this becomes clear bright red. If then saturated with potash, quantities of nitrate of potash crystallize out, with much brown varnish, but no trace of picrate. After one hour's boiling the color of the solution is considerably lighter—the results much the same.

After four hours boiling the color of the liquid was bright yellow. It was evaporated in the water bath and gave a crystalline substance mixed with much resinous matter. To remove this, it was dissolved in as small a quantity of cold water as possible, filtered and mixed with half its bulk of strong sulphuric acid. On cooling, a crystalline reddish yellow substance separated which might easily be taken for picric acid mixed with resinous impurity. But neutralized by ammonia and heated with sulphydrate of ammonia it gave no indications of the presence of picric acid. Tested with cyanid of potassium the results were the same. By spontaneous evaporation of the solution of the substance in ammonia, fan-shaped groups of hair brown needles were obtained. Analysis of these showed conclusively that they consisted of *oxalate of ammonia* disguised by organic matter.

After eight hours boiling the liquid was pale straw yellow and by evaporation on the water bath yielded a substance dissimilar from the former, bright yellow, and colored intensely deep red by cyanid of potassium after previous supersaturation with ammonia. But treated with sulphydrate of ammonia, it gave no indications of the production of blood red picramate, but became greenish brown with production of a greenish precipitate. The presence of oxalic acid could not be detected.

These experiments appear to me to leave no doubt that picric acid is not formed by the action, either brief, or prolonged, of nitric acid on picramic acid, but that resinous substances are produced, accompanied after a time by oxalic acid, which at a later stage, suffers decomposition itself. All of these substances are however produced in very small amount, the greater part of the constituents of the picric acid passing off in volatile decomposition products.

5. *On the Preparation of Picramic Acid.*

We are generally directed to dissolve picrate of ammonia in alcohol, saturate with ammonia, and then with sulphydric acid. These saturations are tedious and troublesome, and as picrate of ammonia is but sparingly soluble in alcohol much of the latter is consumed, and the solutions are very bulky. The following process will be found greatly preferable.

Picric acid (which is very soluble in strong alcohol) is dissolved in cold alcohol, and excess of sulphydate of ammonia added. The liquid then only requires to be evaporated over the water bath, the residue to be exhausted with boiling water, filtered, and treated with acetic acid. The picramic acid obtained in this way is very pure, and the quantity large. In one experiment where the quantities were weighed, over 63 per cent of the weight of the picric acid consumed was obtained. If too little sulphydrate be used, picric acid remains in the mother water from which the picramic acid crystallizes, and may be recovered by precipitating with carbonate of potash.

Philadelphia, July 13, 1861.

ART. XXVII.—*On the Production of new Coloring Matters by Decomposition of Nitronapthaline and Dinitronaphthaline;* by M. CAREY LEA, Philadelphia.

(1.)

IN the process for preparing naphthylamine by the action of acetic acid and iron on nitronaphthaline, the nitronaphthaline is placed in a retort with iron filings and acetic acid, and after the first action has passed off, the contents are heated, a receiver attached and hot lye is added to disengage the naphthylamine. But if a well cooled receiver be attached at the outset of the operation and if heat be applied for some time before the addition of the caustic alkali, a liquid passes over which exhibits the following reactions.

It has a pale reddish color and exhales the disgusting odor of naphthylamine. The pale reddish color becomes pale violet

by addition of mineral acids. If it is placed in an open capsule and heated on the sand bath with addition of dilute sulphuric acid, the pale violet color gradually deepens in intensity to rich blue purple. After a time, a black crystalline precipitate falls which must be separated. The brown filtrate by further heating, again becomes rich purple and deposits a further quantity of precipitate. But eventually the liquid becomes muddy brown (A) and yields no more of the precipitate.

This latter is produced at best in extremely small quantity, and sometimes scarcely appears at all—a grain or two is all that can be obtained from 50 or more gramms of nitronaphthaline.

The properties of this very interesting and beautiful substance, as far as could be determined from the very scanty amount obtained for examination* were as follows. As caught on the filter, it constituted nearly black needles with a most brilliant golden green glitter. After being dissolved in alcohol, it was obtained as a dark red powder, which when placed on glass and a platinum spatula drawn a few times over it, gave a bright green, almost metallic reflecting surface, contrasting strongly with the red powder around it.

It dissolved somewhat readily in alcohol coloring it an intensely deep blood red. The addition of a very small quantity of sulphuric or nitric acid brought this through a succession of shades as the quantity of acid increased, first ruby, then crimson, then rich purple and finally blue purple, all of the richest shades, and so intense as to require great dilution to render the solution at all transparent. The substance exhibits considerable resistance to acids. The alcoholic solution acidulated with sulphuric acid may be boiled without destroying the color; if nitric acid be substituted, the solution by boiling becomes pale straw color, possibly an effect of the reaction of the nitric acid on the alcohol present.

The production of a red color by alkalies and a blue by acids is becoming characteristic of a large number of organic coloring matters. Amongst these are, the coloring matter obtained by Church and Perkins from tincture of madder; by the resinous body obtained by Schiff in the spontaneous decomposition of naphthylurea; by the body obtained by Church and Perkins from nitrosonaphthaline;† by carotin, as observed by Dr. Husemann;‡ by a blue coloring matter obtained from picric acid described by myself. The frequency of this reaction is constantly increasing as we become better acquainted with organic coloring matters.

* Much to the author's regret he was obliged to discontinue this examination in consequence of an unexplained injurious effect upon his health by manipulating with naphthylamine.

† Jahresbericht, 1857, p. 350.

‡ Chem. Centralb., May 1861, p. 347.

The substance which I have here described would doubtless be valuable as a dye if it could be obtained in sufficient quantity, for the richness of its colors leaves nothing to be desired, but it is only a secondary product in the reaction which produces it. Until it can be obtained in sufficient quantity to admit of its constitution being determined, I propose to call it Ionnaphthine, from ἰον, *a violet.**

(2.)

If the muddy brown liquid mentioned at (A) in the 2d paragraph be treated with liquid ammonia, brown flakes separate. If these be treated with dilute sulphuric acid and bichromate of potash they become black. They then do not dissolve in water or alcohol, but dissolve in dilute nitric acid to a deep violet solution, greatly inferior however in color to the solution of ionnaphthine. This substance may possibly be identical with that described by M. Du Wildes† and obtained by him by oxydating napthylamine by means of nitrate of mercury.

(3.)

If the solution of dinitronaphthaline in alcoholic ammonia be heated with solution of sulphite of ammonia, the red solution assumes a rich deep rose color, far richer and more brilliant than the original solution. I have not as yet been able to isolate this substance.

Dinitronaphthaline is as fruitful in colored derivatives as aniline. Treated in solution in alcoholic ammonia with stannous chlorid, it yields a fine blue. Roussin's "artificial alizarine" affords fine shades of purple, the reaction is obtained with great facility. Hofmann and Wood's ninaphthylamine as I have obtained it varies from copper to sealing wax red, but does not seem to me likely to be valuable as a dye. Roussin's alizarine will no doubt be very much so.

ART. XXVIII.—*The Primordial Zone of Texas, with descriptions of New Fossils;* by B. F. SHUMARD.

IN the appendix of Dr. Ferd. Rœmer's volume of Explorations in Texas, published in 1842, we find a notice of Trilobites and other fossils from the valley of San Saba river, which are referred by the author to the epoch of the Lower Silurian system, and subsequently in his *Kreidebildungen von Texas* published in 1852, Prof. Rœmer has given a more extended description of

* The specimen of this new coloring matter which Mr. Lea has sent to us sustains all he says of its beauty.—EDS.

† Rep. de Chimie appliquée, Mai, 1861, p. 172, M. du Wildes is in error in supposing that a reaction which he has obtained is the first instance of a reproduction of an original body from a nitro-substitution compound.

these fossils illustrated with excellent figures. For one of the trilobites Rœmer proposed the genus *Pterocephalia*, which appears to be nearly related, if not identical with *Conocephalites* of Zenker, another and unnamed species possesses all the characters of *Dikelocephalus* of Owen, while a third erroneously figured as the tail of the latter is evidently the head of an *Arionellus*.* It is scarcely necessary to inform palæontologists that these genera are confined exclusively to the Primordial Zone, to which, indeed, Mr. Barrande has already referred the Texan strata, basing his opinion upon the evidence afforded by the work of Rœmer, last cited.

We have no further account of the Primordial rocks of Texas, until 1859, when the present writer published a notice of their discovery in Burnet county (Trans. Acad. Sci., St. Louis, vol. i, p. 673,) in which their parallelism with the Potsdam sandstone and Calciferous sand group of Iowa, Wisconsin and Minnesota and the magnesian limestone series (in part) of Missouri was indicated.

Further explorations have shown that the Primordial rocks with their characteristic fauna are spread over considerable areas in the counties of Burnet, San Saba and Llano, and that they also extend into McCulloch, Mason and Lampasas, and as comparatively little is known respecting their lithological and palæontological features in this, their most southern outcrop hitherto recognized on the American continent, I propose in the present paper to give a brief description of them, drawn from a somewhat careful study of their characters at a number of localities visited by the Geological Survey.†

The Primordial Zone of Texas may be described as a series of light colored, pure and impure dolomites, limestones, chert, calcareous and silicious sandstones, gritstones and conglomerate, presenting an aggregate thickness of from eight to ten hundred feet, and separable into two well marked divisions, of which the superior represents the Calciferous sand group and the inferior the Potsdam sandstone of the northwest.

These rocks are based on reddish feldspathic granite, very similar in lithological character and composition to the granite, which occurs in the region of the Iron Mountain of Missouri, and they are succeeded by even-bedded, hard, brittle, remarkably close-textured, pure limestone and alternating beds of very compact dolomite, sometimes elegantly variegated with delicate flesh-colored cloudings. This formation, some of the beds of which resemble lithographic limestone, has received the name of Burnet marble, and may possibly represent the Bird's-eye limestone of the New York series. The fossils hitherto discov-

* It affords me pleasure to acknowledge here, that in these explorations I have been materially assisted by Prof. W. P. Riddell of the Texas Geological Survey.

† Rœmer's fossils were found in the San Saba valley, McCulloch county.

ered in it are chiefly *Orthoceras* and *Straparollus*, but the few specimens we have found are so badly preserved that they are almost useless for the purpose of identifying the age of the formation.

Calciferous Sand Group.

This formation lies immediately beneath the Burnet marble and consists of calcareo-magnesian limestone, nearly pure limestone and chert. The usual characters of the group may be understood from the following sections:—

Section of Falls of Deer Creek, near Colorado River, Burnet Co.:—

No. 1. Light-colored calcareo-magnesian limestone with much chert, - - - - - - - - 40 ft.

" 2. Alternations of light gray, thick-bedded, compact and subgranular, calcareo-magnesian limestone, and nearly pure white, coarsely crystalline limestone, often handsomely variegated with pink and purple. The upper beds are more compact and of finer texture than the lower, - - - - - 299 ft.

" 3. Very hard, light gray dolomite with a pink tinge, 57 ft.

At Marble Falls, Colorado river, just below the mouth of Flat-Rock creek, we have an interesting section, showing the connection of the Calciferous sand group with the overlying formations thus:

No. 1. Dark, Carboniferous Limestone with Productus semireticulatus (?) Chonetes, Zaphrentis and numerous remains of Crinoidea, - - - - - - - 45 ft.

" 2. Alternations of nearly pure, brittle limestone and dolomite (Burnet Marble), - - - - - - 90 ft.

" 3. Thick beds of very hard, brittle limestone (Burnet Marble), 55 ft.

" 4. Calciferous sand group, consisting of light and dark gray, compact and fine-grained dolomite, in beds from one to six feet thick, - - - - - - - 80 ft.

At Mormon Mills, on Hamilton creek, five miles above its confluence with the Colorado (Burnet county) we find the following section, showing the junction of the formation under notice with the Potsdam sandstone:—

No. 1. Massive beds of sub-crystalline, calcareo-magnesian limestone, variegated with brown and purple and forming rough cliffs, - - - - - - - - 290 ft.

" 2. Gray micaceous sandstone, made up of fine grains cemented with argillo-calcareous matter, - - - - - 5 ft.

Thus characterized the Calciferous sand group of Texas is largely developed in the counties of San Saba, Llano, McCulloch, Menard, Mason and to a limited extent in Lampasas. Its presence may in general be recognized by the rough and picturesque appearance of the country where it prevails.

Along the Colorado for some distance above the mouth of Brady's creek and throughout the whole course of the latter stream in San Saba county and Deer Creek in Burnet county, this formation is characterized by high castellated and mural escarpments, which are strikingly similar in their appearance to the bluffs of 3d or *Lead Bearing Magnesian Limestone* of the Osage, Gasconade, and Niangua in Missouri and those of the same geological age (*Lower Magnesian Limestone*) that escarp the upper Mississippi in Iowa, Wisconsin and Minnesota. In the districts drained by the branches of Morgan's creek in Burnet county, and toward the head of Wallace creek and various other points in the southern part of San Saba, the formation contains much chert, both compact and in the form of large rough cellular masses with cavities coated with brilliant crystals of druzy quartz. The beds are also not unfrequently traversed in various directions by spar veins. In fact they present all the characters of the productive lead-bearing rocks of Missouri and are of the same geological age. It is therefore not at all improbable that valuable deposits of lead will be found within the districts underlaid by these strata in Texas.

Fossils.—In the chert beds of the formation we have found at a few localities *Pleurotomaria*, *Ophileta* and *Orthoceratites* identical with species discovered by Prof. Swallow and the writer in the 3d Magnesian Limestone of Missouri. No fossils have yet been found in the calcareous beds of the formation in Texas.

Potsdam Sandstone.

This important division of the Primordial zone in Texas has a thickness, estimated at not less than five hundred feet. The sections given below taken by the writer in Burnet county, assisted by Dr. W. P. Riddell and S. Heron, Esq., will serve to show the character of the different beds that constitute the mass.

Section at a small creek, five miles N.W. of the town of Burnet:—

No. 1. Soft, chalky limestone with well marked cretaceous fossils, 50 ft.

" 2. Gray and bluish gray, hard limestone, of a sandy texture, in beds from a few inches to a foot thick, and containing *Arionellus* (*Bathyurus* (?) *planus*, *Bathyurus depressus*, *Camerella* sp., *Orthis Coloradoensis*, *Orthis* sp.? *Discina microscopica* and *Lingula* sp.? - - - - - - 81 ft.

" 3. Mottled gray, purple and greenish, earthy and sub-crystalline limestone, with alternating bands of silicious limestone—contains the same fossils as No. 2, - - - - 45 ft.

" 4. Slope with projecting ledges of No. 3, - - 55 ft.

" 5. Slope, - - - - - - - 24 ft.

" 6. Schistose coarse-textured limestone, made up of crystalline particles and abounding with fragments of trilobites, chiefly *Arionellus*, - - - - - 20 ft.

Section taken about one mile south of the preceding at head of Clear creek:—

No. 1. Cretaceous strata in horizontal beds, - - - 110 ft.

" 2. Slope, covered with conglomerate, composed of pebbles of sandstone and calciferous sand rock, rather firmly cemented, 12 ft.

" 3. Potsdam sandstone, consisting of thin layers of variegated green, gray and purplish, sandy limestone, with bands of dolomite and silico-calcareous rock interstratified. Some of the beds are highly charged with trilobites, of which the most common are *Arionellus Texanus*, *Dikelocephalus Rœmeri* and *Conocephalites Billingsi*, - - - - 40 ft.

" 4. Highly ferruginous silicious sandstone, composed of fine grains, loosely cemented, passing downwards into coarse gritstone and conglomerate. The upper beds contain *Lingula* and *Obolus* (?) - - - - - - - 60 ft.

" 5. Flesh-colored granite, interstratified with veins of milky quartz, - - - - - - - 6 ft.

Section near mouth of Morgan's creek:—

No. 1. Coarse-textured, variegated purple, green and brown limestone, soft and crumbling readily to sand, - - - 52 ft.

" 2. Greenish and gray calcareous sandstone made up of fine grains, - - - - - - - 8 ft.

" 3. Slope, with projecting ledges of white, green and gray limestone with *Conocephalites Billingsi*, *Agnostus Coloradoensis* and *Arionellus*, - - - - - - 27½ ft.

" 4. Thin beds of calcareo-silicious limestone with same *trilobites* as No. 3, - - - - - - 60 ft.

" 5. Brown ferruginous sandstone with *Lingula*, - - 60 ft.

" 6. Flesh-colored granite, - - - - 110 ft.

The best exhibitions of this group of rocks occur in the region of Morgan's creek and its tributaries in Burnet county. The remarkable elevation in this region, known as the "Tatur Hill" is composed of this rock. This hill is of truncated conical form, and rises to the height of two hundred and eighty-four feet above the adjacent valley. The Potsdam sandstone is also finely displayed in the southern part of San Saba county and around the margins of the granite districts in Llano. Near the granite the strata are in places highly metamorphosed and disturbed as in the Colorado, near the mouth of Slick-Rock creek, Burnet county, where they are inclined at an angle of nearly 45°.

The fauna of this division of the Primordial Zone of Texas is very analagous to that of the Potsdam sandstone of Iowa, Wisconsin and Minnesota. I have not been able to discover any species common to the two districts but most of the genera are identical. Thus in the Texan beds we have found *Dikelocephalus*, *Bathyurus*, *Arionellus*, *Conocephalites*, *Agnostus*, *Lingula*, *Discina*, *Orthis*, *Camerella*, *Obolus* and *Capulus*, all of which

genera have been discovered in the Primordial sandstones of the northwest except *Agnostus* and *Camerella*.

Compared with the Primordial rocks (Quebec group) of Point Levi, Canada, recently so well described by the Canadian geologists, the Texan strata may be placed on a parallel with the beds provisionally designated as A^2 by Sir W. E. Logan (this Jour., vol. xxxi, p. 216). The propriety of this reference will become apparent by comparing the above-mentioned Texan genera with the following list of fossils given by Logan as occurring in A^2 at Point Levi, viz:—Orthis, Leptena, Camerella, Lingula, Discina, Agnostus, Conocephalites, Arionellus, Dikelocephalus and Bathyurus.*

Descriptions of New Species.

TRILOBITES.

Agnostus Coloradoensis, n. sp.

Head small, nearly circular, convex, length and breadth nearly equal, surrounded with a narrow, neatly defined, flattened border, between which and the dorsal furrows is a small regularly convex surface. Glabella convex, narrow-conical, most elevated posteriorly, occupying scarcely two-thirds the total length of the head, and not quite as wide as one of the cheeks. A single transverse furrow situated near the front divides the glabella into two unequal portions, and at the base on either side is a small triangular lobe. The glabella is strongly separated from the cheeks and front border by a deep furrow.

Length of head, 0·10 of an inch.

A single example only of this species has come under my observation. It bears a close resemblance to *A. orion*, a species recently described by Mr. Billings from the Primordial limestone of Point Levi, opposite Quebec. The *A. Coloradoensis* is, however, a much smaller species, the glabellar furrow is situated much nearer the apex and there is no fissure extending from the glabella to the anterior margin, as occurs in the Canadian species.

Found in Burnet county, near mouth of Morgan's creek associated with *Conocephalites Billingsi* and *Arionellus depressus*.

Arionellus (*Bathyurus*) *Texanus*, n. sp.

Head large, approaching semicircular, wider than long; glabella somewhat regularly convex, conical, distinctly separated from the sides and front by a deep groove; neck-furrow distinctly impressed on the sides and nearly obsolete in the middle; border in front of the glabella broad, occupying about one-third the entire length of the head, and marked posterior to the middle with a deep, and rather wide, transverse furrow, which is parallel with the front margin, and has three deep transverse pits; anterior to this furrow is a broad, smooth space sloping from the front

* The genus *Bathyurus* of Billings appears to me to be closely related to *Crepicephalus* of Owen, and both present many points of resemblance to *Arionellus* of Barrande.

margin to the furrow. A line drawn transversely across the middle of the glabella, if extended would pass nearly through to the centre of the eyes. The facial suture passes forward from the inner anterior corner of the eye, nearly parallel with the axis of the body, until it reaches a point opposite the extremity of the furrow of the front margin, thence obliquely forwards and inwards to the front edge; behind the eye it turns suddenly outwards and slightly backwards and cuts the posterior border near the genal angle.

Pygidium short and somewhat massive, subelliptical, axis approaching semicylindrical, elevated above the lateral lobes, as wide as one lateral lobe, and occupying about two-thirds the length of the pygidium; rings four, separated by deep furrows; posterior margin gently arched in the middle and armed on either side with a long, curved, diverging spine (about eight lines long); lateral lobes gently convex, segments indistinct.

Locality.—This is the largest species of the genus I have seen. It occurs in coarsely crystalline limestone of the age of the Potsdam sandstone, at the head of Clear creek, Burnet county.

Arionellus (*Bathyurus*) *planus* (n. sp.).

Head rather large, approaching semicircular, moderately elevated, having a wide border in front, which slopes from the glabellar furrow to the front margin. Glabella convex, oblong, separated from the cheeks and front by a shallow furrow, elevated above the cheeks, sides straight, slightly converging from the neck segment to the anterior extremity; neck furrow shallow, but distinctly defined entirely across; neck segment convex, arched posteriorly and not as high as the glabella. There are several specimens of the glabella of this species in the Texas State Collection, none of which show any traces of transverse furrows.

Length of head, 0·93; length of glabella, 0·54; width at apex, 0·32; width at base, 0·40.

In general appearance this species resembles *Bathyurus armatus* of Billings, but the glabella of our trilobite is strongly elevated above the general convexity, which is not the case in the Canadian species.

Locality.—In the Potsdam sandstone, head of Morgan's creek, Burnet county, associated with *Orthis Coloradoensis*, near the top of the formation.—Texas State Collection.

Conocephalites depressus (n. sp.).

Glabella not much elevated, most convex in the middle, equal to about two-thirds the length of the head, its contour well-defined by the dorsal furrows; sides nearly straight and gradually converging toward the front, which is rounded, lobation indistinctly marked by obscure lateral furrows, neck furrow well defined but not deeply impressed, border in front of the glabella wide, convex, having an elevated marginal rim in front and just within the rim a distinct groove. Surface in front of the glabella marked with very fine, longitudinal, undulating raised lines; no other surface markings visible on any of the specimens under examination.

Length of the head 0·42 of an inch; length of glabella 0·28; width of glabella at base 0·23, at apex 0·16.

This species is closely related to *C. Wisconensis* Owen, specimens of which I have in my cabinet from the Potsdam sandstone of Lake Pepin, Wisconsin. The glabella is, however, proportionally shorter and the front border wider in the Texan fossil.

Locality.—Head of Clear Creek, Burnet county. Texas State Collection.

Conocephalites Billingsi (n. sp.).

Glabella conical, tumid, occupying about three-fourths of the entire length of the head, about one-third longer than wide, outline strongly defined by the dorsal furrows, which are deeply excavated; neck-furrow strongly impressed, lateral furrows three on either side, deeply impressed, the posterior ones curving inwards and backwards from the dorsal nearly to the neck furrow and reaching rather more than one-third the distance across the glabella, middle furrows curved and reaching scarcely more than one-fourth the distance across—anterior furrows quite short, and shallower than the others; neck segment much elevated, convex, having a small median tubercle which is directed obliquely backwards, and has a small central pit, apparently for the articulation of a slender movable spine; front border occupying about one-fourth of the length of the head, margined in front with an elevated rim, between which and the glabellar furrow is a convex surface. Test thin and apparently quite smooth.

Length of head, 0·33 inch, width of base of glabella 0·16.

Of this well marked species of *Conocephalites* we have discovered only portions of the head of several individuals, consisting of the glabella, and immovable cheeks. They were found in gray coarsely crystalline, thin-bedded limestone near mouth of Morgan's Creek, Burnet county.

The species is named in honor of E. Billings, Esq., Palæontologist of tho Geological Survey of Canada.

Dikelocephalus Rœmeri (n. sp.).

Head approaching semicircular, obtusely subangular in front, having a broad border, with a moderately elevated marginal rim, a groove just within the rim, and between this and the glabellar furrow a convex surface. Glabella convex, elevated, and, including the neck segment, occupying not quite two-thirds the total length of the head; sides straight, slightly converging towards the front, which is moderately arched. The transverse furrows are distinctly marked and divide the glabella into three lobes. The posterior furrow is strongly arched backwards and extends quite across, joining the dorsal furrows a little behind the middle of the glabella. The anterior pair curve backwards, reach about one-third the distance across and are situated in advance of the middle. The dorsal furrows are moderately impressed and distinctly limit the sides of the glabella.

Pygidium short, having a broad slightly excavated smooth border, whose outline is convex on the sides and slightly excavated behind, axis prominent, narrow-conical about as wide as one lateral lobe, excluding the border, contracted at apex, separated from the lateral lobes by a distinct but somewhat shallow furrow; rings five, rather wide, separated by shallow furrows, the last two or three of which are indistinct, lateral lobes

gently convex, sloping very gradually from thoracic margin to border; segments four or five, wide, flattened convex, the anterior one more elevated than the others, posterior ones nearly obsolete.

An excellent figure of the head of this species is given by Dr. Rœmer in his "*Kreidebildungen von Texas*," (Taf. xi, fig. 2, a), but the author has proposed no name for the species. Fig. 2 c–d of same plate belongs to the head of an *Arionellus*, which has been erroneously referred to the pygidium of this species.

Occurs in crystalline gray limestone, of the age of the Potsdam sandstone, at the head of Clear creek, Burnet county, associated with *Arionellus Texanus*.

Named in honor of Prof. Ferd. Rœmer.—Texas State Collection.

BRACHIOPODA.

Discina microscopica (n. sp.).

Shell very minute, conico-subquadrate, much elevated, nearly as high as long; vertex, nearly or quite marginal, and marked with a small pit; area triangular, flattened, extending from apex to posterior margin. Surface marked with extremely fine, concentric lines of growth.

This is an extremely minute species, the largest specimen being scarcely more than $\frac{5}{100}$ of an inch in length.

A few examples of this species were found in the Potsdam sandstone, near the sources of Morgan's creek, Burnet county. It is associated with *Arionellus Texanus* and *Orthis Coloradoensis*.—Texas State Collection.

Camerella (sp.?).

There are several specimens of a small brachiopod in the Texas State Collection, from the Potsdam sandstone of Morgan's creek, Burnet county, which appear to belong to the genus Camarella recently created by Mr. E. Billings. Unfortunately, however, they consist merely of detached and imperfect valves, too much weathered for satisfactory determination and description.

GASTEROPODA.

Capulus (sp.?).

A small, smooth species of *Capulus* with flattened sides, convex dorsum and narrow curved apex, occurs with the preceding at Morgan's creek, Burnet county, but the characters are not sufficiently distinct to warrant us in giving a description of it.

St. Louis, June 12, 1861.

ART. XXIX.—*On the constitution of the Planetary System: and on the theory and tables of Mars, (in a letter addressed to Marshal* VAILLANT); by M. U.-J. LEVERRIER, Director of the Observatory of Paris.*

THE interest which you have taken in the advancement of astronomy and of our knowledge of the system of the world, has induced you to follow with kind regard, my protracted investigations of the four planets, Mercury, Venus, Earth and Mars. It gives me satisfaction to announce to you the completion of these researches;—the last part, relating to the motions of Mars, is finally ended.

The most practical result of these labors is doubtless by comparing theory with observations, to render astronomical tables more precise. Yet the hope of making the tables of the planetary motions absolutely exact, seems insufficient to induce an astronomer to undertake a heavy task of 15 to 20 years, were he not sustained by the thought that he might at least prepare the way for some new discovery:—an ambitious thought, but one which has prompted more than one laborious enterprise. Did not the illustrious astronomer of Königsberg confess that the theory of the Sun had not made the progress which we might have expected from the great number and precision of the observations thereof?

Laying aside long formulæ and tedious calculations, immense series of observations and dry tables of numbers, in short, all professional apparatus, I will here speak only of the results of my labor so far as it relates to the physical constitution of our system.

The existence of the bodies of our system is made known in the most simple manner, when we see them. There is reason to fear, however, that more than one of them may escape us, if, confining ourselves to direct vision as the means of investigation, we do not by some independent method make up for the deficiency of our eyes, even when provided with the most powerful optical apparatus.

The planes of the orbits in which the planets move suffer displacement in the lapse of time, in consequence of the action of masses exterior to the sun; the same is true of the position of the perihelion of the orbits, and the forms of the orbits even undergo alteration. It is evident that the amount of these changes, ascertained by observations, may serve to determine the weight of the bodies which produce them.

Further, were it to be supposed that the only disturbing masses were those of the known planets, it would necessarily result that the variations of all the orbits would agree in giving

* From *Comptes Rendus de l'Acad. des Sciences*, of June 3, 1861.

the same values for these masses; if it shall not, some foreign cause must of necessity have been omitted.

The question presented itself nearly in the following form. Would it be possible by assigning suitable masses to the known planets to satisfy all the observations? Or is there somewhere in our system a notable quantity of matter, which, as yet, has not been taken into account, and of which the consideration is indispensable?

The separate study of each one of the planets affords no answer to this question, whereas the comparison of all the results together enables us to decide with certainty. I will take then the new theory of Mars, and from my previous works, so much as is necessary to the end actually sought, but nothing more.

The position and small size of Mercury and Mars prevent them from exercising any important influence upon the bodies of our system. Observations upon Venus enable us to estimate the mass of Mercury as one five-milionth ($\frac{1}{5\,000\,000}$) of that of the sun; while the motion of the earth, deduced from observations of the sun, indicates the mass of Mars as one three-millionth ($\frac{1}{3\,000\,000}$) that of the sun. The uncertainty which may exist in these numbers has no influence upon that which follows.

The mass of Venus is about one four-hundred-thousandth ($\frac{1}{400\,000}$) that of the sun. This result is obtained by several methods; by the consideration of the displacement of the plane of the ecliptic; by the actual measurement of the periodical perturbations of the earth from 1750 to 1810, and from 1811 to 1850; and by the amount of the periodical inequalities of the longitude of Mercury. These results all confirm each other.

The mass of the earth is one three-hundred-and-fifty-five-thousandth ($\frac{1}{355\,000}$) of that of the sun. This number is derived from a comparison of the force of gravity upon the earth, with the fall of our own planet toward the sun.

These being the data, the theory of Mars may be established by means of them, and compared with meridian observations made a century since, also with observations made upon the near approach of Mars to the star Ψ_2 Aquarii, which were made in the year 1672, at Paris by Cassini and Roëmer, at Cayenne by Bouguer who went there to investigate atmospheric refraction, the obliquity of the ecliptic, and the parallax of Mars.

Now, I have discovered that it is not possible to satisfy in this manner, all the observations of the planet; to do this completely, it is necessary to increase the motion of the perihelion of Mars. This increase, if it could be deduced from a change in the received values of the masses of the planets, could not come from a modification of the mass of Venus (for it does not exert a sufficient influence upon the movement of Mars), but only from an addition to the mass of the earth, an addition which it would be necessary to make equal to the *tenth* part of the value given above.

We shall discuss, farther on, the consequences of this result, confirmed as it is, moreover, by a deduction from the theory of Venus. The increase in the latitude of this planet, leads us to a rigorous condition which can only be satisfied by augmenting the mass either of Venus, or of the earth. In publishing the theory of Venus, I had already remarked that the considerations referred to above, would not allow any change to be made in the mass of Venus; that it appeared necessary therefore, to increase by a *tenth* part the mass of the earth; that in view of the importance of this conclusion, it were better, before deducing the consequences of it, to await the completion of the theory of Mars. Now, as we have just seen, the theory of Mars itself demands that we increase the mass of the earth by *exactly* a *tenth*.

I should remark, before proceeding, that Mercury has already given me a result of the same kind. Observation assigns to the perihelion of this planet a more rapid motion than that which corresponds to the masses above admitted. A change of a *tenth part* in the mass of the earth, will not account for this fact; and as it is impossible to increase the received mass of Venus, I have hence inferred the existence of a ring of masses of matter within the orbit of Mercury. The subject has already been discussed, and will be still farther prosecuted; it may not be unprofitable to repeat the language in which I announced my opinion (*Annales*, t. v, p. 105):

"As a mechanical problem we might satisfy the observed transits of Mercury across the sun, on the hypothesis of a perturbing body, whose place is to be determined. It is however indispensable to consider whether all solutions equally satisfy the physical conditions.

"At the mean distance of 0·17, the disturbing mass would be exactly equal to that of Mercury. The greatest elongation which it could attain would be a little less than 10°. Must we believe that a planet shining with a brilliancy more vivid than that of Mercury, would necessarily have been perceived, grazing the horizon, after the setting, or before the rising of the sun? Or is it possible that the intensity of the dispersed light of the sun would have enabled such a star to escape detection?"

"At a greater distance from the sun the disturbing body must be less considerable, and the same would also doubtless be true with regard to its volume, but the elongation would be greater. Nearer the sun, the contrary would be true, and, although the brilliancy of the disturbing body would be increased in consequence of its size and nearness to the sun, it is possible that a star whose position was unknown, might not be perceived in ordinary circumstances."

"But, even in this case, how could a large body having a great brilliancy and situated always near the sun have escaped being seen during some of the total eclipses? Finally, could such a body pass between the solar disk and the earth, and not be recognized?"

"Such are the objections which might be urged to the existence of a single planet, comparable to Mercury as regards its dimensions, and re-

volving within its orbit. Those to whom these objections appear too serious will be led to replace this single planet by a series of asteroids, the sum of the actions of which would produce the same effect upon the perihelion of Mercury. Aside from the fact that these asteroids would be invisible under ordinary circumstances, their distribution about the sun would prevent their introducing in the motion of Mercury, any periodical inequality of much importance."

"The hypothesis to which we find ourselves conducted, contains nothing extravagant or incredible. A group of asteroids is found between Jupiter and Mars, of which doubtless only the principal individuals have as yet been discovered. There is even reason to believe that the planetary spaces contain an infinite number of very small bodies revolving about the sun. This is certainly true with regard to the region adjacent to the orbit of the earth."

The principal difficulties presented by the system of the four inferior planets, are reduced then to an excess of movement of the perihelion of Mercury, and that of Mars. This two-fold conclusion is worthy of attention; for if the cosmical matter has such a disposition, that although partly or wholly invisible it acts in such a manner as to increase the direct motion of the perihelion, having but little influence apart from this, it is easy to see how the existence of this matter in this condition becomes highly probable.

Now this is, in fact, the mode of action of a series of minute bodies forming a ring about the sun, and revolving from west to east, in the same direction as the other planetary bodies. These bodies, as a whole, could scarcely change the excentricity of the orbit of a planet or cause any sensible periodic inequality in the longitude. Their effect upon the perihelion, however, might become considerable, since it is the sum of the separate effects of each one, so that the final result is sensibly the same as if the whole amount of matter was concentrated in a single mass. Such are the considerations which have induced me to admit the existence of a ring of intra-Mercurial asteroids. The theory of Venus and that of Mars now unite, in confirming these conclusions.

Let us return to the examination of the causes which acting at the other extremity of the system of inferior planets, serve to increase the motion of the perihelion of Mars. They can be accounted for, as we have said, by supposing the mass of the earth increased by a *tenth part.* The motion of Venus in latitude demands the same increase of cosmical matter; but, on the other hand, there is a difficulty with regard to the parallax of the sun.

These demands can all be satisfied and the difficulties all made to disappear by admitting that the asteroids, which, according to observation are situated at the same distance from the sun as the earth have a total mass equal to the *tenth* of that of

the earth. This group of asteroids would accelerate the motion of the perihelion of Mars just as an addition of a *tenth* to the mass of the earth would do. If it is situated very nearly in the ecliptic, it will produce the same effect upon the orbit of Venus. It will have, moreover, no influence upon the periodic terms of the perturbations of Venus and Mars. Finally the relation existing between the mass of the earth, gravity and parallax of the sun will not be altered.*

At the beginning, we had hoped that it would be possible to derive the true mass of the earth from the periodical perturbations of Mars, and from its secular perturbations, some knowledge of the total mass of the asteroids situated between Mars and Jupiter.

The first part of this attempt has only half succeeded, on account of the peculiar circumstances of the observations. There was simply reason to believe it unnecessary to increase the mass of the earth. We see at once what high interest attaches to the direct determination of the velocity of light, and, in consequence, of the amount of the solar parallax. We should thus solve a nice question.

The estimation of the total mass of the small planets situated between Mars and Jupiter, becomes impossible if the group of asteroids which is found at the same distance from the sun as the earth is admitted to have any influence. There being no means of separating completely the effects of these two groups, it is only possible to assign the superior limits of their masses, by attributing to each of the groups successively the whole excess of the movement of the perihelion of Mars. It is thus determined, that the whole amount of matter constituting the small planets situated between the mean distances 2·20 and 3·16 cannot exceed about one-third of the mass of the earth.

The constitution of the inferior (or intra-Jovial) portion of our planetary system, deduced from a discussion of observations, may then be summed up as follows:

1st. *Besides the planets, Mercury, Venus, Earth and Mars, there exists between the sun and Mercury a ring of asteroids whose mass is comparable with the mass of Mercury itself.*

2d. *At the distance of the earth from the sun, is found a second ring of asteroids whose mass is not greater than the tenth part of the mass of the earth.*

3d. *The total mass of the group of small planets situated between Mars and Jupiter is not greater than the third part of the mass of the earth.*

* It is true, that from the determination of the lunar equation of the earth, we have found that both the earth's mass and the solar parallax should be increased. But this result depends on so small a fraction of the lunar equation that we should be fully justified in disregarding it. The final data discussed above have far greater precision.

4th. *The masses of the last two groups are complementary to each other. Ten times the mass of the group situated at the distance of the earth, plus three times the total mass of the small planets between Mars and Jupiter, form a sum equal to the mass of the earth.*

The last conclusion depends upon the determination of the distance of the earth from the sun by observations of the transits of Venus, a determination which astronomers agree in considering as very accurate.

ART. XXX.—*Upon some Improvements, proposed by Sir David Brewster, in the Photographic Camera;* by EDWIN EMERSON, of Troy Univ., N. Y.

SIR DAVID BREWSTER in his "Treatise on the Stereoscope" repeatedly and earnestly insists on the importance of taking the photographic negatives by a lens of very small aperture; he gives as a general rule, (p. 177,) "an aperture as large as the pupil of the eye;" and he makes, also, the following declaration, —"I have no doubt that when chemistry has furnished us with a material more sensitive to light, [than that possessed in 1856,] *a camera without lenses and with only a pin-hole will be the favorite instrument of the photographer;*" in the mean time, however, he suggests that "the use of a lens of rock-crystal, which has a low dispersive power, and having the ratio of the curvature of its surfaces as six to one, with an aperture of one-quarter of an inch, would make an approximatively perfect camera." (pp. 137, 138).

Being convinced, by the excellent results obtained by myself, that a small aperture was indispensable to success in point of sharpness, which was a confirmation of Sir David Brewster's main idea, and having been furnished, by my friend Prof. Rood, with an instantaneous collodion, I began to test by careful experiments, Brewster's notion of a pin-hole camera, and also the use of a quartz lens for the camera, I also endeavored to determine approximatively how small the diaphragm of the ordinary photographic camera should be reduced so as to secure the best results. The instrument in my possession is a very excellent stereoscopic camera, fitted with two portrait combination tubes, and the best achromatic lenses, of Harrison's make. The results were either obtained by this camera or were compared with its work.

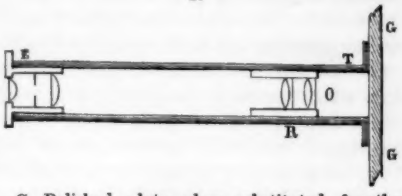
Photographers generally focus upon the ground glass of the camera with the unaided eye, and, of course, in a majority of instances get only an accidental approach to a perfect focus; and when the aperture is over half an inch, for lenses of 6 or 8 inches focus, only a small portion of the whole picture is in focus at all.

Those who use a magnifying glass and a smaller diaphragm succeed better, but by neglecting to reduce the diaphragm to a minimum, their results are by no means as perfect as they might be. By the aid of a common magnifying-glass one and a half inches in focal length, with which to view the image on the ground glass of the camera, and an aperture of two-fifths of an inch in the diaphragm in taking the negative, I was able without any difficulty, to obtain results fully equal in point of sharpness, to the best French transparent views. But in attempting to improve upon this, by reducing the aperture and by increasing the power of the focusing-lens, two difficulties were experienced,—1st. The light was so much diminished that it was well-nigh impossible to see minute shades of difference in the sharpness of definition, and, 2d. The inequalities of the finest ground glass were so much enlarged as seriously to obscure and distort the view projected upon it. This last proved an insuperable bar to any higher degree of excellence by these means, as a single irregularity in the ground glass was magnified quite enough to obscure many fine details. Moreover, with a small diaphragm of the size recommended by Brewster, the light was so faint that a variation of half an inch in the distance of the lenses of the camera from the ground glass, produced no *perceptible* variation in the sharpness of the image. A series of experiments was then made with two objects in view,—1st, to carry up the power of the focusing lens, and, 2d, to subsitute for the ordinary ground glass of the camera something of finer texture so as to stand the microscopic enlargement, and of more transparent quality so as to increase the light. An ordinary "Fothergill plate," covered with the delicate yellowish film of iodid of silver enabled me to carry the magnifying power to fifteen diameters with an aperture of one-fifth of an inch. At the suggestion of Mr. Grunow, of New York, a plain glass plate, lightly covered with dust was tried, which with a power of fifteen diameters rendered practical the employing of an aperture of three-fifths of an inch. But in all the experiments with a single microscope the focus was too indefinite and uncertain for the accurate work I had in view, although the sharpness obtained was far beyond anything I had seen produced by the professional photographers. Indeed, among both British and American artists and amateurs in photography there seems to be a wide difference of opinion as to what may be regarded as *a standard* on this important point. I have not seen a suggestion of the proper means of determining it; which is, undoubtedly, an appeal to the compound microscope.

At this stage of the investigation Prof. Rood constructed for me a compound microscope of a power of thirty diameters, which I hoped to be able to employ as a focuser. Its form and construction will appear from the following diagram, fig. 1.

This instrument I had intended to use upon the focussing plate of plain glass delicately covered with dust; but it occurred to me that possibly plain glass alone would answer to focus upon, and if so, all the obstruction caused by magnified particles of dust, and the want of sufficient light caused by the opacity of the Fothergill film, or anything of a similar character, might be entirely overcome. A trial showed that the amount of light was very greatly increased; but I was now unable to determine the position of the image projected by the lenses of the camera. The lenses of the camera in connection with the lenses of the microscope formed a telescope of considerable power, the lenses reciprocally supplementing each other, so that no matter where the image produced by the lenses of the camera might be formed in space, the microscopic focusser could be readily adjusted to suit it, and give a perfect view. To obviate this uncertainty, fine parallel lines were drawn by a diamond, one-twentieth of an inch apart, upon the surface of the plain focussing-glass nearest to the lenses of the camera; the microscope was then carefully adjusted to distinct vision of these lines, and its lenses fixed in that position; it was now easy to cause the image formed by the camera to coincide in position with these parallel lines, and thus the perfect physical focus for the sensitive plate was absolutely determined, allowance, of course, being made for any difference between the chemical and visual foci, which this arrangement afforded a means of adjusting with the greatest accuracy.

1.



G, Polished plate glass substituted for the ground glass, having fine lines drawn by the diamond on the surface farthest from the microscope.—O, Achromatic objective.—E, Eye-piece.—R, A rack-work at R moves the objective in and out.—ET, Exterior tube with a projection at the objective end at right angles, so as to apply it accurately ro the flat surface of the glass.

By this method of focussing very small objects, such as a single leaf of an elm tree over half a mile distant, were distinctly visible, and were readily focussed upon; and, also, the difference in the focus of objects at a mile, and those at a mile and a half in distance was made apparent; which is utterly beyond the power of the single microscope. The use of a very small diaphragm enabled me now to secure the finest details in all the objects visible in an extended landscape, so that they would bear examination under the compound microscope with a power of 150 to 200 diameters; or, in other words, with a power as high as the structure of the collodion would bear. Up to this time I had supposed that the lenses of my camera were of the same focal length, as very delicate manipulations had failed to detect any discrepancy between them; but by this method of focus-

sing, a variation of nearly one-fifth of an inch was detected immediately. By the use of the instrument the aperture was reduced to one-tenth of an inch, which from careful experiments I regard as about the minimum for good results, for lenses of six inches focus.

I was now prepared to test the ideas of Sir David Brewster, 1st, with regard to a small aperture, 2d with reference to a rock-crystal lens, and 3d, as to a pin-hole alone substituted for a lens, in connection with a very rapid collodion. By means of the binocular camera I was able to make two experiments simultaneously, on the same plate, the left-hand view being taken by the portrait combination, and the right-hand view being taken with a quartz lens, or a common glass lens, or a pin-hole, as the case might be. The scenes or objects photographed were always exposed to the full light of the sun; and the plate, in each pair of experiments, was, of course, subject to a uniform treatment, under similar conditions as to collodion, baths, &c. I was thus able to compare the results of the various experiments side by side with the action of the achromatic combination; as follows—

Portrait combination, 6 inches focus.

1. One second exposure, $\frac{1}{4}$ inch diaphragm. A pretty fair negative.
2. Thirty seconds exposure, $\frac{1}{4}$ inch diaphragm, good negative, would stand a magnifying power of 30 diameters.
3. Two minutes exposure, $\frac{1}{10}$ inch diaphragm, strong negative, view sharp all over the field, would stand a magnifying power of 100 diameters.
4. Two and a half minutes exposure, $\frac{1}{12}$ inch diaphragm, negative good, but would not stand a power of over 50 diameters.
5. Pin-hole diaphragm, could see nothing distinctly to focus upon, four minutes exposure, very faint impression on the sensitive plate.
6. Pin-hole diaphragm, seven minutes exposure, strong negative, but no fine details.
7. Common glass lens, 6 inches focus, $\frac{1}{10}$ inch diaphragm, two minutes exposure, view sharp, would stand 30 diameters, tolerably.

Quartz lens, 6 inches focus.

1. One second exposure, $\frac{1}{4}$ inch diaphragm. Action slightly quicker, negative pretty fair, but less sharp.
2. Thirty seconds exposure, $\frac{1}{4}$ inch diaphragm. Action slightly quicker, more contrast, but would not stand a magnifying power of 5 diameters.
3. Two minutes exposure, $\frac{1}{10}$ inch diaphragm, no quicker action, no stronger negative, and would not stand a magnifying power of 5 diameters.
4. Two and a half minutes exposure, $\frac{1}{12}$ inch diaphragm, no quicker action, negative would only stand a magnifying power of 4 or 5 diameters.
5. Pin-hole diaphragm. Similar result.
6. Pin-hole diaphragm. Similar result.
7. $\frac{1}{10}$ inch diaphragm, two minutes exposure, quartz action quicker than the glass, more intense negative, but not as sharp, owing possibly to a slight double refraction in the lens.

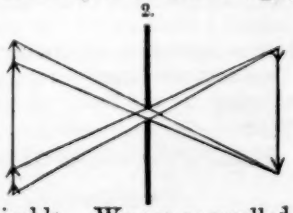
Portrait combination, 6 inches focus.	*Pin-hole alone.*
1. One second exposure, one inch diaphragm, a good negative.	1. One second exposure,—result nothing.
2. Thirty seconds exposure, half inch diaphragm, a good negative.	2. Thirty seconds exposure, result almost nothing.
3. One minute exposure, $\frac{1}{4}$ inch diaphragm, good sharp negative.	3. One minute exposure,—faint shadowy outline of a picture.
4. Two minutes exposure, $\frac{1}{10}$ inch diaphragm, excellent negative, details of objects with a mile good under a magnifying power of 100 diameters.	4. Two minutes exposure, middling contrast between light and shade but exceedingly blurred in all the details.
5. Three minutes exposure; picture over-exposed, with $\frac{1}{10}$ inch diaphragm.	5. Three minutes exposure, view of a white bust in the sun-light, good contrast of light and shade, picture only of the prominent outlines looked like a very rough mosaic.
6. Three minutes exposure, $\frac{1}{12}$ inch diaphragm, good negative, but not sharp enough for a power of 50 diameters.	6. Seven minutes exposure, negative strong enough to print from; but no sharpness at all; details blurred and confused.

The results of these experiments may be summed up as follows:—

1. It is found in practice that a quartz lens is quicker in action than a common glass lens of the same aperture, focus and thickness.

2. A quartz lens is very slightly quicker than the double combination of achromatic lenses; but it will not afford any thing like so sharp details even under the smallest diaphragms. "A quartz lens camera with $\frac{1}{4}$ inch aperture" is, therefore, very far from being "an approximatively perfect camera."

3. The simple pin-hole will not compare under any circumstances with the commonest glass lens; as it gives no sharpness to the picture, and requires from three to seven minutes exposure, with the most sensitive collodion. This lack of sharpness might have been predicted theoretically. To give sharpness to the image, the pin-hole must be as small as the smallest detail in the view required, say the ten-thousandth of an inch, otherwise the rays of light from a single point travelling such paths as are indicated in the diagram produce necessarily a blurred image; thus—(fig. 2,) i. e., sharpness will be in proportion to the smallness of the pin-hole; now as a pin-hole of the $\frac{1}{10}$th of an inch in diameter requires with the most sensitive collodion an exposure of several minutes, it is apparent that a hole the ten-thousandth of an inch in diameter would require an exposure entirely impracticable. We are compelled,



therefore, to regard Sir David Brewster's pin-hole camera as an optical as well as a photographical absurdity.

4. The diaphragm for lenses 6 inches in focus, can be reduced down to about an aperture of one-tenth of an inch with a very decided improvement in sharpness, if the compound microscope is used to focus with on a plain glass focussing plate; but when we reduce the aperture below this there is a loss of sharpness; owing, it may be, to the approximate parallelism of the rays.

5. A good use for a pin-hole camera would be as a simple instrument for testing the sensitiveness of collodions, and I would recommend it for this purpose.

Troy University, August, 1861.

ART. XXXI.—*On the Age of the Red Sandstone formation of Vermont;* by E. BILLINGS.*

I HAVE lately been examining a tract of the Calciferous sandrock which lies on the boundary line between Canada and Vermont on Missisquodi Bay. The rock is exposed here in long parallel ridges, over an area of eight or nine miles in length and from one to three in width. On the east side of the exposure there is a ridge of greyish sandstone which I traced south across the boundary line, after crossing which it soon becomes interstratified with thick beds of rock of a chocolate red or brown color. It is here the typical red sandrock formation of Prof. Adams. Hearing that Dr. G. M. Hall and Rev. J. D. Perry of Swanton had discovered trilobites near this place I called upon them and they kindly conducted me to the locality. It is above two miles south of the line and one mile or a little more east of the Highgate Springs. The individual fossils are abundant in the red sandstone but I could find only two species, a small *Theca* and a *Conocephalites*. Of the latter we found only the head but the specimens are very numerous and some of them well preserved. The species resembles Bradley's *C. minutus* but is a little larger and I think quite distinct therefrom. It is a true primordial type and if we are to be guided at all by Palæontology we cannot regard this rock as lying at the top of the Lower Silurian but at the very base of Barrande's Second Fauna if not indeed a little lower. It is therefore not the Medina Sandstone but a formation somewhere near the horizon of the Potsdam. This accords exactly with conclusion drawn from the evidence afforded by the fossils discovered by our survey at Quebec last year.

Museum of the Geological Commission, Montreal, Canada, July 22, 1861.

* In a letter to one of the Editors of this Journal.

ART. XXXII.—*Agricultural Chemistry—Soil-analysis: Notice of the Agricultural Chemistry of the Geological Surveys of Kentucky and Arkansas;** by Prof. S. W. JOHNSON of Yale College.

IN no country has there been so much popular appreciation of practical science as in the United States of America. Scarcely one of the States is without its volume or volumes of Geological and Natural History Reports, and though some of them have been content to confine the work to the merest outline of the general and industrial geology of their territory, and have expended but a few hundreds of dollars in the undertaking, others, like New York, have embraced all the branches of Natural Science in their survey, have prolonged the work of exploration or elaboration through many years, and have devoted money to these objects with unsparing hand.

The results of these surveys as they stand recorded in the numerous volumes published by the States and by the General Government, are of very unequal merit, as might be expected from the wide range of country explored, from the various degrees of interest and appreciation governing the many Legislatures which have authorized these labors and from the exceedingly unequal ability of the individuals charged with their execution.

These explorations have originated in all cases with our scientific men. It is their influence either brought to bear immediately upon the legislative bodies, or exerted less directly through cultivated and public spirited persons to whom the possible benefits of geological surveys have been explained—that has accomplished this vast work.

The enterprises of which we speak being sustained pecuniarily at the expense of the people, and depending from year to year in many cases upon the popular vote, it has been not only politic but right to exhibit at the outset the prospects of pecuniary return for the required outlay of means, as an inducement to support such undertakings. It has been no less proper in presenting the results of the surveys, to lay stress on the discoveries having industrial bearings which are the fruits of the work.

In those States where large quantities of metallic ores occur, the interest of capitalists engaged in mining has often sufficed to inaugurate a geological survey. In other states the agricultural sentiment has had to be operated upon.

Great results have been promised to agriculture from the applications of geology and chemistry, and a great deal of labor has

* 1st, 2d, 3d and 4th Reports of the Geological Survey of Kentucky 1854-60: 2d Report of the Geological Reconnoisance of Arkansas, 1860: Agricultural Chemistry and Geology by Dr. D. D. OWEN, principal Geologist, and Dr. ROBERT PETER, Chemical Assistant.

been performed in the attempt to satisfy the hopes that have been thus excited.

The chief object of the present notice is to inquire what has been really accomplished for the good of the farmer, by the scientific surveys that have been hitherto prosecuted in this country.

The labors of Dr. Peter in connection with the Kentucky and Arkansas Surveys being the most recent and extended attempts of this kind, we shall make them the basis of our inquiries.

If we except a few pages of general remarks on the theory of vegetable nutrition, &c., which while useful to the practical readers of the Report contain no new facts or principles,—the whole effort of Dr. Peter has been concentrated on the analysis of soils, marls, rocks and ashes. He publishes in the four Kentucky Reports analyses of 375 soils, and in the Arkansas Report, 187, in all 562 soil analyses. Besides, we find the results of examinations of 145 rocks, shales, &c., and of 38 ashes of plants, making a grand total of 795 agricultural analyses.

The agricultural fruits of the surveys of Kentucky and Arkansas are then to be sought in these analyses.

It certainly will strike all that the amount of work performed by Dr. Peter is unusually great. It is now but six years since the Kentucky survey was commenced and in that time the Dr. has not only analyzed 795 soils, but has executed 516 analyses of ores, slags, mineral waters and coals, making an average of two analyses for every three days of this whole period. This labor Dr. Peter states he has accomplished with the help of one intelligent assistant, and by a special organization of his laboratory and his operations whereby the utmost economy of time was secured. We have had such experience of the advantages of a similar system, that we are not prepared to doubt that the chemist who adopts a plan of analysis which fully satisfies him, and from which he never departs, may execute such an amount of work. At the same time we must bear in mind that the only control Dr. Peter offers for the accuracy of his results is, that the sum of the weights of the separated ingredients equals their original conjoined weight, no time being allowed to repeat a determination, or to prove the purity of a precipitate.

The Analytical Process followed in these analyses is not by any means so minute and full as we should be warranted to expect, when their author declares (4th Ky. Rep., p. 57) that "such a work to be eminently useful must be thorough and exhaustive;" for soluble silica, chlorine, nitric acid and ammonia are not at all estimated, and the condition of the iron, whether protoxyd or peroxyd, is not noticed. It is worthy of notice that carbonic acid and lime are always present in atomic proportions in the soils latterly analyzed, no excess of either ingredient being mentioned in the results. Carbonic acid however is not noticed

in the description of the analytical process, and that figuring in the analyses does not appear to have been directly estimated, but to have come from the oxalic acid of the reagent shelf.

If, as might easily happen, the contrary not being proved, a portion of the lime dissolved by hydrochloric acid exists in these soils as silicate, sulphate or phosphate, then the assumption that it is united to carbonic acid introduces an error into the summing up (which in many cases is exactly 100) and shows that a quantity of some other ingredient has been overlooked.

For the estimation of phosphoric acid a highly modified form of Sonnenschein's process is employed, but our author does not give the figures which prove that his changes are improvements.

Admitting however that the analyses are correct—we next inquire what is their value—what useful deductions from them appear in these Reports.

In the introduction to Vol. i, Kentucky Survey, page 13, Dr. Owen says: "By consulting the numerous interesting results obtained by the chemical analyses of the soils embodied in the pages of this report, abundant evidence will be gathered of the vital necessity of wide dissemination amongst the farming community, of the knowledge to be obtained by a correct insight into their chemical constitution." In the same volume, page 373, Dr. Peter remarks that he was impressed "that when the composition of our Kentucky soils and minerals in general, is once accurately established, their applications to our wants and uses would be obvious to all well informed persons. He has therefore consumed the time mainly in the analyses, and made up his report principally of the results."

In the agricultural section of the Arkansas Survey, p. 47, Dr. Owen says:—"principally from chemical soil-analyses can the agriculturist form an intelligent opinion as to the comparative fertility of soils, and their suitability to the growth of certain plants, as well as judge what applications may be required in the way of lime, bone earth, plaster of Paris, ashes, or salts of potash, soda, &c."

Dr. Peter, in the same volume, page 166–7, observes:—"It is believed that by no other mode than by chemical analysis or by the more tedious and laborious method of actual experience, in cropping for a series of years and publishing a record of the same, can the actual nature, capabilities and value of the various soils of a State be presented to the public; and that by instituting this Geologico-Agricultural Survey, the State of Arkansas not only aids materially in the progress of the general science of the civilized world, and that of the soil in particular, but takes the most effectual mode of making known to the enlightened immigrant her agricultural riches. In this she has followed the wise lead of the older state of Kentucky, in which, since the institution of her geological survey, the value of the land

in the regions examined and reported on has been very greatly enhanced."

In the Agricultural Geology of Kentucky, Report 2d, p. 9, Dr. Owen says: "Placing implicit reliance on the capabilities of chemical science to indicate by the analyses of soils, the ingredients removed by the cultivation and harvesting of successive crops, it was hoped that by collecting samples of the virgin soil, and of the same soil from an adjacent old field, that not only the different substances assimilated out of the soil could be ascertained, but also the exact proportion of these so that the farmer might know precisely what must be restored to the land to bring back its original fertility."

These quotations sufficiently show what were the opinions which led our author to devote such an amount of labor to the analysis of soils, and indicate in general, what results were expected.

In the 2d Arkansas Report, p. 49 et seq., Dr. Owen "proceeds to explain in what way soil-analysis becomes of value to the farmer." He desires "to call particular attention to this subject, because the opinion has been expressed even in this year (1860), and by those having a high standing in the scientific world, that chemistry is incapable of conveying any useful information to the farmer by analyzing his soil."

On the six following pages of the 2d Ark. Rep., and on page 30 of the 4th Ky. Rep., Dr. Owen gives the most complete *résumé* of the teachings of soil-analysis which we are able to find in the five volumes before us, and as these are his latest writings on the subject, and as he then had the data of 389 analyses, viz. of 187 Arkansas soils and 202 in the three volumes of the Kentucky Report,—these being refrered to on the pages we are quoting from,—we are warranted in considering what he has here presented, as embodying the *strong points* in favor of soil-analysis. We will notice them separately as gathered from both Reports.

1st. "Any one who will take the trouble to inspect the analyses of the 187 Arkansas soils will see that the relative proportions of the eleven mineral constituents of these soils is very accurately given."—*2d Ark. Rep.*, p. 49.

If we admit fully that Dr. Peter's analyses represent with fair accuracy the composition of the two grammes of soil he experimented with in each instance, we do not therefore allow that the composition of "these soils" considered as representing geological formations, or large agricultural districts, or even single fields, is "very accurately given."

Here at the outset the distinguished gentlemen who have conducted the 'geologico-' and 'chemico-agricultural' part of the Kentucky and Arkansas surveys have taken for granted, what being an error, overturns their whole reasoning, and renders their soil-analyses comparatively worthless.

Years ago, following the teachers of agricultural chemistry in this country and England, we believed that soil-analyses were adapted to be of exceeding use to farmers. Having practised analytical chemistry sufficiently to undertake the work, we proceeded, when on a vacation visit, to collect some farm soils for the purpose of applying our skill and knowledge. On putting down the spade and post-augur into the drift overlying the lowest Silurian of Northern New York, we were at once struck with the difficulty of procuring an average specimen. The soil for a depth varying from two to six inches was quite fine, but below that depth largely mixed with gravel. On comparing different samples taken from a small area, it was plain that the soil was not a fit subject for analysis. The relative quantities of organic matter as indicated by the color of the surface of small stones,—some quartz and granitic, others slate and limestone of several geological members,—were astonishingly variable. Here we found the soil sandy, there it was clay. To take a sample from one place was to do obvious injustice to the sixty-acre field. To take it from a dozen places would not render the selection of a fair sample any more certain. Then as to depth —was it proper to go down six inches, one foot, or how far? Had the field been a bed of iron ore, assays of a dozen samples taken from different parts would have indicated very satisfactorily the general value of the deposit, would have served as data for buying and selling the property, because the worth of an unworked bed of such ore depends less upon its content of iron than upon external circumstances which affect the extracting of the metal. Had the field been covered with rich dressed copper ore to the depth of six inches, it would have been necessary to divide it up into small parcels of a few tons, average these carefully and as carefully assay each one. No one would risk purchasing a hundred thousand tons of copper ore on the analysis of one or of a dozen samples, because it is impracticable to intermix or average such a mass of material as that a dozen samples shall accurately represent it.

We hold it therefore as the first objection to soil-analyses that to procure a specimen which accurately and *certainly* represent a field or district, is practically impossible in the majority of cases, *and if possible, requires a series of analyses to prove the fact.*

This argument applies with the greater force when we consider how small a proportion of the ingredients of a soil are of any immediate use in feeding crops. The really active nutrient matters of a soil are not reckoned by per cents nor by tenths of per cents, but by the minutest fractions.

A heavy crop of thirty-seven bushels of wheat, grain and straw included, removes from an acre of land but 300 lbs. total of mineral matters. According to Dr. Peter's weighings on some

of the Kentucky soils, we may assume, that taken to the depth of a foot, an acre of soil weighs 3,000,000 lbs. All that is removed by the heaviest wheat crop then in one year is but $\frac{1}{30,000}$, or 0·0033+ per cent.

It follows that the annual removal of the heaviest crop of wheat from a soil for 100 years diminishes its mineral matters by less than 0·4 per cent. If then, in the selection of a sample, the average composition is departed from to the amount of 4 parts in 1000, the analysis may misrepresent the soil, by the value of 3700 bushels of wheat per acre, or by what represents, so far as mineral ingredients can, the fertility of a century.

What freaks and accidents is not the soil-analyst the sport of? A bird, squirrel, or dog relieving nature at the spot where he collects his sample, innocently magnifies the phosphoric acid or alkalies of the surrounding thousand acres a hundred fold. The soil gathered toward the end of a long rain, whereby its soluble matters are carried deep into the subsoil, is declared poor, by analysis, whereas if taken after a fortnight of drought it might appear extraordinarily fertile. Boussingault found in his rich garden soil in June, during wet weather, 0·00034 per cent of nitric acid. In the following September, after a period of dryness, it contained 0·0093 per cent, or twenty-seven times as much as in June. This ingredient is indeed more liable to fluctuation in amount than any other, both because it is formed in the soil, and because it is not subject to the absorbent action which the soil exercises over most other of its soluble constituents; but the same variation occurs among the other ingredients according to the direction of the capillary movement of the soil-water, though in less degree.

Independently however of all considerations and calculations like the above, we have proof—evidence at least that supports these considerations, and has never been publicly refuted—that it is practically impossible to obtain average specimens of the soil. I refer to investigations made as long ago as 1846–9 under the direction of the Prussian "*Landes Oekonomie Collegium*," and reported by the distinguished Magnus. The account of these experiments is given in detail in Erdmann's Journal für Praktische Chemie, vol. xlviii, pp. 447 et seq.

The "*Landes Oekonomie Collegium*" at that time carried on systematic experiments in agriculture at fourteen distinct stations scattered through the Prussian domain. The trials which we now speak of, were made for the ostensible purpose of studying the exhaustion of the soil by cropping. The plan was to analyze the fourteen soils, the history of which for years previous was accurately known, then crop them with rape until "exhausted," then compare together the original composition of the soils with their composition after exhaustion, taking into account as well,

the composition of the crops removed. The research began with collecting and analyzing the soils. In order to meet as far as possible the difficulties of securing average specimens, equal portions of the soil of each field were taken with the spade at ten or twelve different points, and thoroughly intermixed; each sample was then passed through a sieve, the holes of which were two square lines in area, so as to remove all coarser stones, then again well worked over to complete the mixture. Of each sample three separate portions were analyzed, in most cases by different operators. The analyses were made by, or under the guidance of, the ablest chemists of Germany. They were made according to a prescribed scheme, and that there should be no reason to slight the work, the work was paid for. It is true that analytical chemistry was not so advanced in 1846 as now. It is true that the methods then practised for estimating phosphoric acid and some other substances were not as perfect as they now are; but for the most part the analyses then made are as accurate as any that could be executed to-day. It cannot be supposed for a moment that analysts like Rammelsberg, Bödecker, Genth, Debus, Knop, Heintz, Krocker, Marchand, Weidenbusch, Sonnenschein, Varrentrap, Weber, &c. &c., would by fault of method or by carelessness return anything but results that were accurate, as far as it was possible to make them such. We cannot suppose that their determinations of lime, oxyd of iron, potash and sulphuric acid, substances estimated then by the same methods that are now employed, would vary if they were supplied with homogeneous material to operate upon. But let us look at some of their figures. We tabulate a number of them taken at random:

Soil of		Lime.	Potash.	Sulphuric acid.	Phosphoric acid.
Eldena,	*a.*	0·39	0·93	0·08	0·06
	b.	0·75	2·06	——	0·17
	c.	0·25	0·12	0·02	0·40
Beesdau,	*a.*	0·802	3·825	——	0·042
	b.	0·039	0·490	——	0·046
	c.	0·715	0·792	0·004	0·007
Neuensund,	*a.*	1·692	3·531	0·050	0·051
	b.	0·614	1·289	0·038	0·010
	c.	0·728	1·243	0·241	0·121
Turwe,	*a.*	2·312	1·112	0·040	0·057
	b.	2·67	1·14	0·03	0·20
	c.	3·391	0·201	0·022	0·014
Frankenfelde,	*a.*	0·420	1·155	0·016	0·004
	b.	1·081	——	——	0·418
	c.	0·461	1·456	0·015	0·071

If we run over these figures and those of the entire series of analyses, we find that different determinations disagree to such an extent as to make it the sheerest folly to base any calculation

of the value of the soil upon analysis. Some of the analyses agree sufficiently to show that accordant results are possible if uniform material be taken; but the grand result of the investigation is that the difficulties of getting a uniform material are exceedingly great. Again, we must remember that in the case before us, the three examinations of each soil were made upon portions of one carefully mixed sample. What would have been the result had each chemist received a sample collected separately from all the others, and from different parts of the field!

Dr. Peter mentions these analyses of the *Landes Collaeium*, and quotes a few of the results on page 187 of the 3d Kentucky Report. He believes however that these discordant results do not invalidate soil analyses when made as they may be made with "means and appliances *now* at the service of the analytical chemist" and thinks "this statement however hazardous it may seem will be found to be sustained" in his Report.

In the Report before us however we do not find anything to sustain Dr. Peter's view. He gives, so far as we have discovered, no duplicate analyses, to show what accuracy his methods admit of on the same sample, much less does he prove by analyses of specimens separately gathered from the same field, that it is easy to procure an average material for analysis. Until this proof is produced the evidence is in favor of our view.

Having shown how small an error in sampling may affect the chemist's estimate of a soil, it is not out of place to insist for a moment, that a similar error in the analysis itself, must have the same result. In running over 200 pages of Dr. Peter's 4th Kentucky Report, we find five analyses of soil in which there is a gain of from five to eight tenths per cent; we find twenty-three in which there is a loss exceeding five tenths per cent. In thirteen of the latter the loss is eight or more tenths, in eight instances the loss is one per cent or more, and in one case is one and eight-tenths per cent. We should scorn to notice little matters like these, errors which are inseparable from the best manipulation and the best processes, were it not that in soil analysis it is precisely the small quantities which alone have any importance.

We find in Dr. Peter's work, as in the work of all who have preceded him in the analysis of soils from Davy and Sprengel down, evidence that the best endeavors in this line of research are entirely incommensurate with the desired results.

It may be objected to this criticism of the analyses that the loss or gain must be distributed among the twelve ingredients determined. It is true that there is a probability that such distribution would be just; but this is by no means *certain*, and it is equally true that this being done there is still force in the criticism—for the four-tenths per cent of the soil which a cen-

tury of wheat crops would remove, likewise consists of twelve ingredients.

The 2d result of these analyses, according to Drs. Owen and Peter, is what the former (4th Ky. Rep., p. 33) declares to be "a general law" "now established," viz., "that *soil-analysis is capable of showing the exhaustion in land of the mineral food of plants by continual cropping.*"

To show the removal of soil-ingredients by cropping, the plan was followed of collecting soils from contiguous fields, one of which had been "cultivated" while the other was in its virgin state. On comparing the analyses it was found that in seventy-one* cases out of seventy-nine, a loss had occurred in the soil which had been in use without manure from ten to fifty years. In eight instances, however, the analysis failed to show such a result, owing to local causes, the soil of the old field being based on a sub-soil richer than was the virgin field, or the old field having received washings of more elevated lands, &c.

The admitted richness of the old over the new soil in these eight exceptional cases, is expressed by hundredths of per cent, e. g., soil Nos. 982, virgin, and 983, cultivated, differ by 0·066 per cent of potash. Soils 1144 and 1146 by 0·032 per cent of phosphoric acid. Soils 1204 and 1205 by 0·092 per cent phosphoric acid. Soils 1207 and 1208 by 0·033 per cent potash. Similar fractions likewise show the amount of deterioration in the other seventy-one cases. We adduce two instances pointed out by Dr. Peter in the 3d Kentucky Report, p. 207, and one given on p. 176 of the 2d Arkansas Report:

	Carb. of lime.	Magnesia.	Phosphoric acid.	Potash.
Virgin soil, No. 557,	0·345	0·335	0·181†	0·156
Old soil, No. 558,	0·215	0·465	0·103†	0·101
Difference,	0·130	0·130 gain.	0·078†	0·055
Virgin soil, No. 738,	0·180	0·444	0·179	0·256
Old field, No. 739,	0·145	0·388	0·163	0·179
Difference,	0·035	0·056	0·016	0·077
Virgin soil, No. 288,	0·121	0·371	0·127	0·116
Old field, No. 289,	0·021	0·371	0·053	0·097
Difference,	0·100	0·000	0·074	0·019

We were prepared to find these differences much larger. It is seen at a glance that they fall within the errors of Dr. Peter's own manipulation, and when we assert that of ten analyses of the most homogeneous material made by the same analyst under the most favorable circumstances, five would differ among each other by an amount equal to the quantities upon which this "natural law" is supported, we assert what every competent

* Misprinted *twenty*-one, on p. 31, 4th Kentucky Report.

† Misprinted on p. 207. 3d Ky. Rep., where the difference is made 0·045 instead of 0·078, as given above from the tabulated analyses.

analyst knows to be true, and what moreover pronounces most emphatically upon the value of such investigations.

It is therefore our conclusion, that while, as has long been known, the soil loses in mineral matter what the crop gains, it is doubtful if in any given case chemical analysis can indicate this difference with certainty, for the reasons that the accidents which affect analysis make the limits of inaccuracy, to cover more than the loss by years of cropping. When we take into account the changes that are constantly progressing in the soil when under cultivation—changes by which the disintegration is hastened, changes by which it is made in many instances more retentive of soluble matters—when we remember that most cultivated crops, although they carry off in seed, stem and foliage a quantity of mineral matters, yet derive these in part from a depth below the range of analysis, and in their roots or stubble, leave upon the surface, salts brought up from a considerable depth—we perceive that the problem is so complicated with compensations and variable quantities as to put it beyond the reach of quantitative chemical analysis.

If, in any case, soil-analysis does show or appear to show the exhaustion of the soil, it is however, the appeal to experience which *proves* it, and as this is the first, most obvious, and an entirely sufficient proof, we do not see the value of the "law" that has 10 per cent (eight-seventy-ninths) of exceptions, the existence of which like that of the rule itself, is only to be established by comparison with the plain agricultural fact.

In short, if we admit the result as Drs. Owen and Peter would have it—of what use or interest is it?

The 3d point, is that analysis shows "the peculiarities of the soils derived from different geological formations." Says Dr. Owen, "these analyses most distinctly show that certain geological formations impart to the soil more of the important mineral fertilizers than others." The reader will be able "to see that it is those formations which are composed of easily disintegrating materials, which, all other things being equal, yield the soils richest in phosphoric acid, lime and potash; and at the same time contain the quantity of alumina and oxyd of iron necessary to render them sufficiently retentive and attractive of atmospheric water and ammonia; therefore these soils are the best adapted for those grains and crops which require the largest proportion of these ingredients." "He will moreover be able to trace the gradual diminution in the proportion of the more important mineral ingredients, down from these extraordinarily fertile soils derived from the highly fossiliferous, argillo-calcareous beds of the lower Silurian, the Cretaceous and the Tertiary systems of the West; through the silico-calcareous soils of the upper Silurian, Devonian and Sub-Carboniferous limestone strata, in which fossils are either more sparingly distributed or, in some

cases almost wanting, and which are far less easy of decomposition; thence through the argillo-silicious soils of the Coal measures with only locally organic remains, and these chiefly of plants, down to the more purely silicious soils prevalent where the non-fossiliferous sandstones of the Coal measures and of the Millstone Grit, prevail to the exclusion of either shales or limestones and which afford the most unproductive soils as yet analyzed." While it is to be expected that rocks of complex origin rich in organic remains—which are evidences that the rocks themselves originally resulted from the deposition of the washings of fertile lands—should yield richer soils than sandstones or limestones, we do not see that analysis of the soil makes the fact more evident Knowledge of the composition of a rock enables us to judge in a general way of the value of the soil, so far as this depends upon chemical characters. We do not see what is gained by further analyses of the soil. It would appear that the cheap mental processes of deduction or inference may accomplish here in a moment all that an expensive analysis can show.

We fail moreover to perceive that analysis shows "the *peculiarities* of the soils derived from the different geological formations." In a cretaceous or limestone soil we of course expect to find much carbonate of lime, and in a sandstone or millstone grit soil much insoluble silica or silicates, but the quantities of phosphoric acid, potash and sulphuric acid do not appear to bear any definite relation to their geological origin. It is impossible to represent the composition of the soil of any geological formation by a typical statement of percentages, or to point out its peculiarities further than by an undefinable more or less. Although Kentucky and Arkansas lie mostly or altogether beyond the influence of drift, yet the action of running water in its constant passage from hill-top to valley has to a great degree obliterated from the soils those *peculiar* differences to be found among the rocks from which they have been derived.

A careful examination of the analyses recorded in the Arkansas survey shows that the average composition of the eight soils analyzed from the Lower Silurian and of the fourteen from the millstone grit, compare as follows, in regard to the more important ingredients:

	Carb. lime.	Magnesia.	Phosphoric acid.	Sulphuric acid.	Potash.
Lower Silurian, average of 8 soils,	0·533	0·485	0·184	0·052	0·355
Millstone grit, " 14 "	0·215	0·531	0·180	0·057	0·148

Here we see that the soils of the poorest formation are inferior to those of the richest only in carbonate of lime and potash. Of the soils of the millstone grit, nine are richer in carb. lime than the poorest of the Silurian, and five of the former contain more potash than the poorest of the latter. On the other hand but two of the Silurian soils have higher percentages of either carb.

lime or potash, than the richest soil of the millstone grit. If these figures demonstrate anything, it is the fact, that no geological formation has the absolute monopoly of either barren or fertile soils. If the analyses of Dr. Peter show the "peculiarities" of the soils of any geological age, then certainly these peculiarities are not remarkably peculiar!

On page 50 of the 2d Ark. Rep., Dr. Owen remarks as follows:

"With the table of the composition of the ashes of plants to refer to, appended to this Report, and after becoming acquainted with the usual proportions of mineral constituents in an average soil, information which is easily acquired by looking over the table of soil analyses in this Report, it is easy for any individual to see, when he is provided with a reliable analysis of his soil, not only to what crop it is best adapted, but what kind of mineral fertilizers, if any, it requires as a manure, and how it compares in fertility to the various grades of soils from other farms and other states. Is not this knowledge of some value to the farmer?"

The above, we are of opinion, proceeded rather from the generous heart than from the critical brain of its lamented author. Had he attempted to *do* the things which he believed to be so easy, we are sure his statements would have lost somewhat of their directness and would have appeared in a form highly modified from the above. "The usual proportion of ingredients in an average soil." What is an average soil? Our only way of deciding what is such a soil consists in noting the average yield of soils. But the yield depends not alone on the soil, but upon climate, weather, tillage and various incidents and accidents. It depends not on the composition of the soil—not on the "proportion of ingredients" alone, but likewise on the condition of those ingredients, their state of combination, their solubility. It depends also on the physical characters of the soil, which determine the relations of the crop to the essential conditions of regulated heat and moisture. The soil is not less important to the plant in its function of home than in its function of food, the lodgings are of equal influence with the board. It is a nice work to balance these varying circumstances, many of which have as yet in our science, no shadow of a numerical expression, and then to say how many thousandths of a per cent of potash, lime, phosphoric acid, &c., belong to the "average soil."

Dr. Peter has indeed attempted to show the degree of availability of the elements of the soil by the following process. "A quantity, generally thirty grammes of the air-dried soil is placed in an eight-ounce strong vial, with a close fitting stopper, and the bottle is filled up with distilled water which has been charged with pure carbonic acid gas, under a pressure of about two atmospheres. The bottle is allowed to remain for about a

month at a temperature about that of summer heat." The matters thus dissolved were then analyzed as usual. These results have this value, they show that the water of the soil is capable of dissolving all the elements of the food of plants. They furnish moreover a *rough* comparative view of the available matters in different soils. Beyond this we cannot attach any value to them.

We now come to Dr. Owen's 4th result of soil-analyses, embodied in the above quotation, and repeated on p. 30 of the 4th Ky. Rep., viz: its power of indicating "the suitability of the soil for any particular crop." Closely related to this is the 5th item, viz., that analysis can show "what addition any soil, either uncultivated or cultivated, requires to render it productive and remunerative for any given crop; and, of course, the deficiency in the soil of one or more of the eleven elements determined by chemical analysis."

We cannot help feeling that the above assertions which are here made unqualifiedly, were intended to be understood with a large amount of reserve and subject to various conditions. Otherwise we must regard them quite unjustified, if not absurd. The chemical analysis of soil reveals nothing as to its tenacity or lightness, its porosity or retentiveness for water, yet these physical and mechanical conditions more than anything else determine the adaptation of a soil for any particular crop. The best grass lands are not the best wheat lands—and although it would scarcely be questioned that wheat requires a richer soil than grass in order to produce an average crop, and although as we know, it often happens that many successive hay crops may be removed from a meadow without sensible diminution of the yield, while uninterrupted cropping with wheat nearly always reduces the capacity of the soil in a very few years below a profitable point; yet each average hay crop removes from a field more of every ingredient of vegetation than the grain and straw together of an average harvest of wheat.

Such at least is the testimony borne by the most recent and trustworthy data. Dr. Anderson of Glasgow basing his calculations on the best analyses and on the extensive agricultural statistics gathered in late years by the Highland and Ag. Society of Scotland, makes the following estimate of the amount of the principal ingredients removed from an acre by average crops of seven staple British farm products. See table on next page.—*Trans. Highland and Ag. Soc.*, 1861, p. 568.

On comparing the amount of matters removed from an acre by the wheat and hay crops, we find that the latter requires four times as much potash, lime and sulphuric acid; twice as much silica and one-fifth more nitrogen.

Again we know that oats are raised on soils which are considered too poor for the profitable production of wheat, and the

	Produce per Imperial Acre.	Total Weight in lbs.	Total Mineral Matters.	Potash.	Soda.	Lime.	Magnesia.	Chlorine.	Sulphuric acid.	Phosphoric acid.	Silica.	Nitrogen.
Wheat—												
Grain, ...	28 bush. at 60 lb.	1680	34·12	10·11	1·20	1·04	4·80	....	0·32	16·22	0·43	29·20
Straw, ...	1 ton, 3 cwt.	2576	114·48	20·70	2·84	8·53	2·23	....	3·55	3·16	73·47	16·13
Total, ...		....	148·60	30·81	4·04	9·57	7·03	....	3·87	19·38	73·90	45·33
Barley—												
Grain, ...	33 bush. at 53 lb.	1749	44·24	9·40	0·30	0·76	3·10	1·12	0·85	15·52	13·19	34·98
Straw, ...	18 cwt.	2016	99·14	11·24	1·14	5·81	2·75	1·30	1·10	7·22	68·58	6·03
Total, ...		....	143·38	20·64	1·44	6·57	5·85	2·42	1·95	22·74	81·77	41·01
Oats—												
Grain, ...	34 bush. at 40 lb.	1360	48·89	11·00	....	5·31	4·04	0·20	....	26·07	2·27	27·54
Straw, ...	1 ton.	2240	143·53	30·71	6·10	10·29	5·50	5·55	5·18	7·35	72·85	14·10
Total, ...		....	192·42	41·71	6·10	15·60	9·54	5·75	5·18	33·42	75·12	41·64
Beans and Peas—												
Grain, ...	25 bush. at 60 lb.	1650	55·97	30·00	0·31	3·01	4·00	....	1·76	16·65	0·24	46·10
Straw, ...	1 ton.	2240	108·51	48·61	13·14	29·37	3·74	7·00	2·07	0·74	3·84	26·88
Total, ...		....	164·48	78·61	13·45	32·38	7·74	7·00	3·83	17·39	4·08	72·98
Turnips, ...	13½ tons.	30,240	213·75	57·35	44·71	28·60	4·65	10·35	39·02	22·57	6·50	60·48
Potatoes, ...	3 tons.	6720	55·58	28·92	2·85	1·20	2·11	3·21	10·24	5·76	1·29	26·00
Hay, ...	2½ tons.	5600	391·31	129·79	4·80	35·46	9·62	39·61	16·57	21·79	133·67	56·22

table shows us that an average crop of oats requires more of every mineral ingredient than is needful for a corresponding wheat crop.

In fact, wheat is the crop, to grow which continuously requires, according to universal agricultural experience, land richer than that needed for any other of the seven crops whose chemical statistics are given in Dr. Anderson's Table, and notwithstanding, with exception of barley and the potato-tuber, it removes the least from the soil.

The farmer knows that wheat delights in a deep, rather heavy soil, one which holds moisture well, and yet is not wet. Barley and oats flourish on soils that are too dry and light, and grass on those which are too wet for wheat.

But how does the matter stand when these external conditions are taken into account? Does not analysis aid us then in a good degree? Let us take a case similar to what has repeatedly occurred in actual practice. We have a soil which as the result of long cultivation or from natural deficiencies, is incapable of yielding a remunerative crop of wheat. Its texture is good, it has produced wheat abundantly, and needs nothing but a little of the right kind of manure to restore its power of giving a crop. We put upon it Peruvian guano at the rate of 300 lbs. per acre, and the harvest is a good one. The entire addition to the soil is but $\frac{3}{3\,0\,0\,0\,0\,0}$ths = one hundredth per cent. The amounts of phosphoric acid, of alkaline earths and nitrogen added, are for each, but one six-hundredth per cent of the soil, taken to the depth of a foot. These quantities are rather minute for even the improved analysis of the present time to estimate successfully.

Calculations like this show that the chemist cannot discriminate by his analysis between; 1st, a soil which is unproductive from the temporary exhaustion of some of its available ingredients; 2d, the same soil which is rendered fertile again for a year by the use of 300 lbs. of guano; and 3d, the same, made over-rich so that nothing will grow on it, by an application of a ton of guano.

On page 18 of the 2d Ky. Rep., Dr. Owen remarks as follows: "During last summer a soil was collected in Bullit county, from an old field which had been fifty or sixty years in cultivation, and which will now no longer produce clover. I venture to predict that when the analysis of this soil shall be completed it will be found to be deficient in some of these constituents,* and the analysis will probably show what other green crop might succeed better for the renovation of such land."

On page 230 of the 3d Ky. Rep., Dr. Peter gives the analysis of this soil, and says, "The inability of this soil to produce clover

* The mineral ingredients of plants.

is explained by its very small proportion of lime, and rather small amount of sulphuric and phosphoric acids. The addition of plaster of Paris or some of the calcareous marls would probably restore it to the capability of supporting a clover crop."

The percentages of the ingredients which Dr. Peter considers deficient, are as follows:

Carbonate of lime,	- - -	0·072 = lime 0·040
Sulphuric acid,	- - -	0·055
Phosphoric acid,	- - -	0·070

Small as are these quantities, the smallest of them, viz., that of lime, yet amounts to 1200 lbs. per acre, which is enough to supply 10 clover crops of 3 tons each, and as by the analysis it all exists in the form of carbonate, it must all be available. We know from the vegetation experiments of Boussingault, Ville, and Sachs, that plants are capable of absorbing from a limited amount of soil *the whole* of any soluble nutritive substance present, provided its quantity be no more than the plants require, and the other elements of fertility are at hand in excess.

Twelve or thirteen years ago, Dr. Anderson in his capacity of Chemist to the Highland and Ag. Society of Scotland, had occasion to investigate two soils which had become "clover-sick," and he caused them, together with similar adjacent soils which still produced clover, to be most minutely analyzed. Without reproducing his figures, which may be found in the Trans. of the Highland and Ag. Soc. for 1849–51, p. 204, we will merely quote some of the remarks which accompany the analyses: "The results of these analyses are certainly of an unexpected character, and appear to me to indicate that, in this instance the failure of the clover cannot have been dependent upon the chemical constitution of the soil. In both cases the results of the analyses of each pair do not present a greater difference than would be obtained from the analyses of two portions of soil from different parts of any field."

In the present year, Stoeckhardt (*Chemischer Ackersmann*, No. 2, 1861, p. 85), has published an account of several "clover-sick" soils from Schlanstaedt, which reveal to analysis a greater content of *every nutritive mineral ingredient* both soluble in water and in acids, than exists in another soil from Frankenstein which produces clover and wheat as well. What proves beyond a doubt that the inability of these soils to yield clover depends upon something besides their chemical constitution, is the fact that lucerne and esparsette still flourish upon them admirably, and further, clover itself, if sown with one of these last mentioned crops, succeeds very well.

A great truth in agriculture is this: Each kind of agricultural plant requires that its seeds be surrounded with certain conditions in order that they may germinate readily and healthfully, so

that when the mother cotyledons are exhausted, the young plants shall attack the stores of food in the soil with that vigor which is needful in order to appropriate them without hindrance.

The fact that winter wheat is more delicate and fastidious in its infancy than most other crops, is perhaps the main reason why it does not succeed well on many good lands, and why it cannot be continuously produced from the same soil, year after year. It is a matter of experience that wheat requires a rather firm seed-bed: beans, oats and mangold-wurzel approach wheat in their requirements, while barley, peas and turnips are best suited in a light tilth. On the other hand, climate, weather and tillage so influence the character of the soil, that even on light lands, wheat may find all the conditions of its growth. The bed which is produced by inverting a clover sod, and allowing it to consolidate by time and rains, or by passing a heavy roller over it, is eminently adapted to wheat, even on a rather light soil.

The fact that in the cases given above from Stoeckhardt, clover succeeded when sown with lucerne or esparsette, would indicate that, possibly, the condition of the seed-bed was the cause of failure.

These and other facts which might be adduced to almost any extent, indicate sufficiently that chemical analysis alone, even if we admit its full nicety and accuracy, can at the best furnish us with a knowledge of but a few of many conditions which must coöperate in profitable agricultural production, and as a consequence, its part in guiding the farmer is but very subordinate. Taking into the account its evident uncertainty and clumsiness when applied to estimating the minute quantities which affect vegetable growth, the part it can play becomes still more subordinate—we hesitate not to say, insignificant.

As we write, a fragment from a Scientific Journal brings to our notice a discovery which if real, strengthens our views in an unexpected manner. It is well known that iodine is so immensely diluted in sea-water—the soil of marine-plants—that none of our tests though they are among the most delicate, serve to detect it directly, and it is doubtful if it has been detected even in the highly concentrated mother liquors which remain after separating the crystallizable salts, yet the fuci find and accumulate it, and we must grant that it is present there for them, in sufficient quantity.

Again, Prince Salm Horstmar several years since, in his admirable researches on the influence of the individual mineral ingredients of plants on the development of oats and barley, found that he could not by any possibility exclude chlorine from his experimental plants. His soils and pots, the salts and water he fed his plants with were so purified that he could not detect this element in them, and yet he invariably discovered it in the

ashes of the plants. So too he found titanic acid in the produce grown on the most carefully purified soils. Now, it is mentioned in the "Chemical News" that he finds *a few hundredths of lithium are indispensable* to the ripening of barley. This element Bunsen has but recently shown to be everywhere distributed, yet it has been hitherto entirely unnoticed in all soil- and plant-analyses because of its occurrence in almost infinitesimal quantity.

It must be well borne in mind that Agriculture herself—so-called Practice—is able of her own resources to judge somewhat of the value of soils, is able to know if a soil be fertile or poor, is able to pronounce upon its adaptation to crops, and can to a certain extent decide what is a good manure for this or that field.

We are free to assert that the knowledge which is now to be gathered from experience, is able in ninety-nine cases out of one hundred, to give a more truthful verdict as to the capacity of a soil, that any amount of analysis, chemical, mechanical or otherwise, can do. We would give more for the opinion of an old intelligent farmer than for that of the most skilled chemist in most questions connected with farming. Doubtless the farmer would make some blunders from which chemistry might save him, but the chemist would be likely to do more violence to agriculture, than the farmer would to chemistry.

By these statements which may, but should not surprise some of our scientific friends, we merely intend to express an opinion as to the present relative position towards agriculture of those who regard the art from a chemical, and those who see it from an experimental point of view.

If any one has fuller and more inspiring notions of the importance of science in its applications to agriculture than we have, we desire to sit at his feet and share the higher *afflatus*. But our inspiration, if it be of the sort that works enduring benefit, must be based on clear ideas of the directions in which advance is possible and on a full perception of the difficulties that lie before us, and the means of overcoming them.

We have great faith that chemistry and that chemical analysis have done and are to do a work for agriculture, that shall lay that venerable art under everlasting obligations to the youthful science. But not by soil-analyses alone or mainly is this to be achieved. We do not assert that soil-analysis is worthless—we believe that the probabilities of its uselessness in direct application to practice are so great that we would rarely base any operations on it alone, and yet it may in many cases, promote science and give us data for conclusions that are of practical use. But for these purposes it must form part of a system of observations and trials, must be a step in some research, must stand not as the index to a barren fact, but as the revelator of fruitful ideas.

We hold that soil-analysis long ago played out the part which Dr. Peter would have it perform. In the hands of Sprengel it

was fertile with new truth, but it must henceforth be a tool for occasional use, and not an engine of discovery. With our advance in knowledge there must be an advance in methods of finding out the unknown. Soil-analysis was indeed a means of insight into the secrets of vegetable growth, but it carried with it the measure of its limit. What we call telescopes do not enable us to see *the end!*

To study the soil in the hope of benefitting agriculture, we must regard all its relations to the plant. We must examine it not merely from those points of view which theoretical chemistry suggests, but especially from those which a knowledge of practical agriculture furnishes. This is becoming more and more the habit of agricultural chemists and the results are of the happiest kind.

Let us remember what the illustrious Nestor of Agricultural Science, Boussingault, has said as the summing up of his protracted experience and study.

"At an epoch not far distant it was believed that a strict connexion existed between the composition and the quality of arable soil. Numerous analyses shortly modified this opinion as too positive. The sagacious Schübler even sought to prove in a research that has become classic, that the fertility of a soil depends more upon its physical properties, its state of aggregation, power of absorption, &c., than upon its chemical constitution."

"The physical properties, in my opinion, do not enable us, more than the chemical composition, to pronounce upon the degree of fertility of the soil. To decide this point with some measure of certainty, it is indispensable to have recourse to direct observation; it is necessary to cultivate a plant in the soil, and ascertain with what vigor it developes there: the analysis of the plant afterward intervenes usefully, to indicate the kind and quantity of the elements that have been assimilated."—("*De la Terre Végétale considerée dans ses Effets sur la Vegetation*," page 283 of "*Agronomie, Chimie agricole et Physiologie, Tome premier*, 1860.")

There has been much progress made in our knowledge of the soil during the last ten years. This advance has not consisted in revealing to us the presence of new elements (lithia perhaps excepted), nor in fixing with any more certainty the quantitative limits which separate barrenness from fertility, it has not shown what is the composition of a Silurian or a Sub-Carboniferous, a Drift or a Tertiary soil, it has not defined the soil adapted to wheat or that productive of clover, it has not indicated the manures which this or that soil needs; but content with the fact that all soils which naturally support vegetation contain the elements of vegetation, it has sought to ascertain in what forms these elements are assimilable, how they may be made available, what changes or reactions in the soil affect its productiveness; how fertilizers act indirectly (their influence often having no

relation to any supposable direct action), how the soil affects the life of the plant otherwise than by feeding it, &c. &c.

We are approaching in fact by slow degrees to an understanding of the physiological significance of the soil, a grand result to which chemistry and physics coöperate.

We trust that in the future, the American people will not less but more appreciate the value of science in its practical and especially its agricultural bearings; that here, as in Germany, France and England, the labors of those who seek to unite Practice with Science may be fostered and sustained. But to this end scientific men must be cautious that in endeavoring to help, however honestly and laboriously they may work, they do not hinder.

Sheffield Scientific School, August 20th, 1861.

Art. XXIII.—*The Great Comet of* 1861.*

1. *Observations at New Haven.*

On Sunday evening June 30th between 8 and 9 o'clock, there was observed at New Haven, in the northern part of the heavens in an opening between the clouds, and at an elevation of about ten degrees, a nebulous body of unusual brilliancy. Its appearance was similar to that of the planet Jupiter shining through a thin mist; and it was nearly as conspicuous an object in the heavens as Jupiter, although this was due not wholly to the intensity of its light, but partly to its extent of surface, its apparent diameter being about equal to that of the full moon. It was at once suspected that this body was a comet; but this conclusion was adopted with some reserve, on account of the unusual brilliancy and sudden apparition of the meteor. This light was soon concealed by a cloud; but about half an hour later, a larger opening in the clouds disclosed the tail of the comet, in the form of a bright streamer, with sides nearly straight and parallel, and pretty sharply defined. The head of the comet was now invisible; but a little later, both head and tail were seen simultaneously, forming together one of the most brilliant comets of the last fifty years, and astonishing every one by the suddenness of its development. Mr. R. W. Wright of this city marked the position of the comet's head upon a star chart as accurately as he was able, and hence concludes that about a quarter before nine o'clock, June 30, its R. A. was 108° and Dec. 47° N.

On Monday it was ascertained that on Saturday evening, several individuals had noticed in the north a bright streamer rising to a great height above the horizon, and it was at once concluded

* The first part of this paper was published in advance of this issue, July 20th. We now reproduce it with important additions.—Eds.

that this was the tail of the same comet. The daily newspapers report that the head of the comet was seen on Saturday evening at Columbus, in Ohio, but it is not known that any one made any accurate determination of its place.

On Monday night at New Haven the sky was overcast; but on Tuesday evening July 2d, the sky was mostly clear, and the comet very conspicuous; although it was thought that its head was not as brilliant as it had been on Sunday evening. At $9^h\ 31^m$ P. M., the position of the head was in R. A. $8^h\ 41^m$, and Dec. 63° 5′ N. Seen through a telescope of five inches aperture, with a power of 55, the head was fully thirty minutes in diameter. Near the center of the nebulosity appeared a very brilliant nucleus, from which emanated a luminous sector, whose opening was about 90°, one side being nearly vertical and the other or right side was nearly horizontal. This brush of light extended two minutes from the nucleus. The tail of the comet could be traced to a distance of 90° from the head.

On Wednesday evening the sky was again clear, and the comet was observed to great advantage, but its brilliancy had palpably declined since Sunday evening. At $9^h\ 5^m$ P. M., its head was in R. A. $9^h\ 52^m$ and Dec. 66° 10′ N. Seen through the telescope, the coma had about the same extent as on the preceding evening, but the luminous sector already mentioned, had changed very noticeably. The sides of the sector were curved, the concavity being outwards, and the opening of the sector amounted to 136° when measured to the extremities of its arc, but the initial directions of the two sides formed an angle at the nucleus of about 90°. From the nucleus to the edge of the sector was 1′ 34″. Beyond this, there was a dark arch or band concentric with the nucleus, and beyond the dark band a luminous arch or envelope faint and misty, the middle line of which was 2′ 56″ from the nucleus. Beyond this there were faint indications of a second envelope, with an intervening dark arch, the whole forming a series of nearly concentric light and dark arches, similar to those observed in Donati's comet in 1858, and in Halley's comet in 1835. The tail of the comet on Wednesday evening could be traced through an arc of 95°, and the deviation of its axis from the position of direct opposition to the sun was about 12°, and toward the east, the axis produced cutting the ecliptic about 8° behind the sun's place.

The tail of the comet was carefully observed on several clear evenings, but the observations were more detailed and complete on the evening of July 3d. The northern edge grazed the star Lambda Draconis, passed about 15′ to the south of Kappa Draconis, and continued on through Iota Draconis, and far beyond it, in an arc of a great circle. The southern edge passed just to the north of H. 32, Ursae Majoris, grazed H. 30, Ursae Majoris, and continued on through the stars 3 and 8 Draconis. (Accord-

ing to the B. A. Catalogue these stars are Nos. 3496, 3358, 3968, and 4347.)

It broke off, or suddenly became faint, before it reached the distance of Alpha Draconis, at about 20° from the nucleus. From that point the tail continued as a much fainter milky band, decreasing very gradually in luminosity, and varying but little in apparent breadth. This breadth was less than one-half the breadth of the extremity of the brighter portion, which was about 3°. The southern edge of the narrower and fainter stream passed through Alpha Draconis, and grazed the stars Tau, Sigma, and Eta Herculis. The decreasing light of this stream vanished in the immediate vicinity of the Milky Way, to the east of β Ophiuchi. The extreme length of the tail was about 95°. The train of the comet was apparently made up of two distinct streams of luminous matter, differing greatly in width and length. The northern edges of the two were in the same line, but the extreme breadth of the shorter stream was much greater than that of the other. Its southern edge was badly defined, and somewhat concave outward. A very faint diffused light, rapidly widening out, could be traced far beyond the point where the sudden falling off of brightness occurred. This diffused light extended, on the evenings of July 4th and 5th, to the vicinity of Corona Borealis, or more than 40° from the nucleus, and attained to a width of 12° or 15°. Its southern edge passed just to the north of the star Theta Bootis. The breadth of the tail, as distinctly seen, at its broadest part, was about 3°. On the evening of June 30th, the estimated breadth was 5°; but a faint light on the south side was traced 5° farther, giving an extreme breadth of 10°. On July 4th, the tail was visibly forked about 2° below the star Alpha Draconis, or 15½° from the nucleus. On the following evening the point of forking was 3° or 4° above the same star. The nucleus had advanced 5½° toward it in the interval.

It was also observed, on the evening of July 4th, that by examining carefully it could be discerned that the long narrow stream increased in breadth about in proportion to the distance from the nucleus. At the point where first seen as a distinct stream its breadth was about 1½°.

Since July 5th the tail of the comet has decreased, from night to night, in brightness, as well as in length and breadth.

2. *Observations at Washington.*

The following places of the comet as observed at the U. S. Naval Observatory, Washington, have been communicated by Lieut. J. M. Gilliss, Superintendent of the Observatory. The observations were made by Mr. Ferguson with the large equatorial.

M. T. Washington.	No. of comp.	α	δ
1861, July 2, $9^h\ 55^m\ 19^s{\cdot}3$	10	$8^h\ 43^m\ 6^s{\cdot}97$	$+63°\ 12'\ 14''{\cdot}7$
3 $8\ 46\ 46{\cdot}3$	2	$9\ 51\ 41{\cdot}15$	$66\ 9\ 52{\cdot}5$
3 $9\ 10\ 7{\cdot}3$	5	$9\ 52\ 52{\cdot}55$	$66\ 11\ 6{\cdot}0$
4 $8\ 51\ 20{\cdot}5$	12	$10\ 58\ 36{\cdot}26$	$66\ 54\ 20{\cdot}6$
6 $9\ 0\ 52{\cdot}5$	4	$12\ 31\ 12{\cdot}57$	$64\ 51\ 7{\cdot}6$

On the night of the 3d, it was observed on the meridian with the transit instrument by Prof. Robinson, U. S. N. and with the mural circle by Prof. Hubbard.

M. T. Washington.	α	δ
July 3 $15^h\ 21^m\ 46^s{\cdot}7$	$10^h\ 11^m\ 7^s.25$	$66°\ 33'\ 31''{\cdot}5$

From the observations of the 2d, 4th and 6th, the following elements of its orbit were computed by Prof. Hubbard.

Perihelion passage 1861. June 11.43955. Washington mean time.		
Longitude of the perihelion,	$249°\ 11'\ 28''{\cdot}4$	Mean eqx.
Longitude of ascending node,	$278\ 58\ 32\ {\cdot}1$	1861·0
Inclination of the orbit,	$85\ 37\ 35\ {\cdot}5$	
Perihelion distance,	0·821531	
	Motion direct.	

These elements give for the middle observation

$$\Delta l = -15''{\cdot}7\ (c-o)$$
$$\Delta b = -10\ {\cdot}6$$

It is obvious from these elements, that this comet is not the same as the comet of 1556 (called Charles Vth's comet) whose return has been anticipated for several years; nor do these elements bear any resemblance to those of any comet in the published catalogues. We must conclude then that this comet is a new one, whose orbit has never before been computed.*

3. *The Comet, as seen at the Observatory of Harvard College, Cambridge, Mass.* Communicated by G. P. Bond, Director.

The comet was first seen at the Observatory of Harvard College in the early twilight on the evening of Tuesday, July 2d. The sky was clouded on the first and on the thirtieth. On Saturday the twenty-ninth of June the air was hazy preventing the usual sweeping for comets, although observations near the meridian were prosecuted until about 11^h P. M. Had the sky been clear, the tail of the comet would probably have been seen. A day or two previous, the western twilight had been explored with an opera glass, but at this time only the upper part of the tail could have been in sight, and it must have been too faint to attract notice.

The condition of the theory of cometary formation makes it very desirable that astronomers should devote more attention than they have hitherto been accustomed to do, to the accurate

* We have received from Lt. Gilliss a paper on the physical characters of this comet, which will appear in November. Prof. Hubbard's latest computations however are given on p. 265.

delineation of the curve of the tail among the stars. The present opportunity has been improved at the Observatory of Harvard College by making careful tracings of the boundaries of the rays through their entire extent upon star charts. The *Uranometria Nova* of Argelander was found to be especially convenient for the purpose, both from the exactness of the projection and the care taken in giving the proper magnitudes to the stars which greatly facilitates their identification. An uninterrupted series of clear nights from the second of July to the present time has very much favored us in preserving the continuity of the phenomena, which is a condition of the utmost importance for their future discussion.

The suddenness of the apparition of the comet in northern latitudes was one of the most impressive of its characteristics. On the 2d of July after the twilight had disappeared, the head, to the naked eye, was much brighter than a star of the first magnitude, if only the effective impression be taken into account, although as to intensity it was far inferior to α Lyræ, or even to α Ursæ Majoris. I should describe the head as nearly equal in brightness to that of the great comet of 1858 between the 30th of September and the 5th of October; it should be considered however that the present comet was better situated, from its higher position above the horizon at the end of twilight.

The aspect of the tail suggested a resemblance to the comet of March, 1843. It was a narrow, straight ray projected to a distance of one hundred and six degrees (106°) from the nucleus, being easily distinguishable quite up to the borders of the milky way. The boundaries for the most part were well defined and easily traced among the stars. It was not until after two or three hours of observation, that I could gain a clear comprehension of the structure of the tail or tails as they presented themselves to the naked eye and through a small opera glass. It was then evident that a diffuse, dim light with very uncertain outlines, apparently composed of hazy filaments, swept off in a strong curve towards the stars in the tail of Ursa Major—the southern edge directed as low as towards Mizar. This was evidently a broad curved tail, intersected on its curved side at the distance of a few degrees from the nucleus by the long straight ray which at the first glance, from its greatly superior brightness, seemed alone to constitute the tail. The two were in fact counterparts of the principal tail and the supplementary rays of the great comet of 1858, with this remarkable difference, that in the latter the straight rays were so far inferior in brightness to the curved tail as to have been recognized at only three observatories, those of Poulkova, Göttingen and Cambridge, U. S.—while with the present comet, the predominating feature was the straight ray to which the curved tail seemed scarcely more than a wisp-like appendage.

On further scrutiny with the aid of an opera-glass, two sharply cut and very narrow dark channels, bounding the principal ray, could be traced for ten or fifteen degrees from the nucleus; while outside of them, on either side, were two additional faint rays. The whole issue of nebulous matter from the nucleus far into the tail was curiously grooved and striated. It was noticed that both the principal ray and the dark channels penetrated within the outline of the curved tail, the latter being clearly separated from the principal ray even to the naked eye by a dark cleft just above their intersection. The well defined margin of the principal ray admitted of a very exact delineation, even as far as α Ophiuchi, 100° from its origin.

On the third, the bright rays and dark channels were traced to a distance of 40° from the nucleus, the principal ray to nearly 100°. Five or six alternations were distinguished, besides the hazy filaments constituting the curved tail. Some of the streaks could be traced quite up to the nucleus. The rays were not only separated by the dark channel parallel to their axes, but they were disconnected at intervals in the direction of their length.

On the fourth, there were two or more regions of contrary flexure on the north following margin of the ray, which, in a theoretical point of view, are of very great interest when taken in connection with the direction of the ray almost precisely in a great circle from the sun continued through the nucleus. This peculiarity presented itself still more decisively on the fifth, when the tortuous path of the ray could not be overlooked.

The very singular aspect of the northern edge of the principal ray for the first thirty or forty degrees of its course, attracted particular attention, and the charts were revised with all possible care. The sky was perfectly clear and the outlines so distinct that there could be no room for doubt as to the reality of the reflexure of the curve. Subsequently on projecting an arc of a great circle from the sun through the nucleus, it was found to lie clearly within the margin of the ray as far as a distance of thirty degrees (30°), from the nucleus, and there was still haziness beyond it almost to the distance of sixty degrees (60°). The charts on other dates indicate similar results, but the data cannot be properly discussed without requiring more labor than can be, at present, devoted to them.

Within the last few days the principal ray in the part near the nucleus, has assumed a more regular sweep in the direction opposed to that of the diffuse tail, which now reaches nearly to the centre of Corona Borealis, scarcely changing the course of its southern limit between κ and ι Bootis and ζ Coronæ Borealis from night to nigltt.

The telescopic phenomena, though interesting, have not presented equally strongly defined features with those which char-

acterized the great comet of 1858. We should perhaps except from this remark their structure for a day or two after their first emission from the nucleus. In this stage they were intersected by jets of luminous matter projected from the nucleus, and these limits were pretty clearly outlined.

On the 2d, portions of three were visible; the inner one showing a variety of details. In its outline and general aspect it was, like others which followed it, almost a fac-simile on an enlarged scale of some of those exhibited by the great comet of 1858. They rapidly faded, or were lost in the surrounding haze and their places were filled by new ones. Latterly, two, at most, could be seen at one time. It is quite important to remark that the successive envelopes resembled their predecessors not only in their general aspect but quite closely in the details of their structure; the luminous jets not issuing at random from all points alike of the nucleus, but continuing to follow a nearly similar course at each new discharge from its surface.

The most natural inference from this would seem to be that the nucleus, if it rotates at all upon an axis, does so very slowly. Of the pendulum-like vibrations of the luminous sectors ascribed by Bessel to the comet of Halley, nothing was seen; although the opportunity of witnessing them, had they existed, was very favorable, as the sectors were well displayed.

The nucleus was throughout brilliant, and, to appearance, solid, with a diameter of from 2″ to 3″.

The disposition of the nebulosity in the part of the tail contiguous to the head was nearly uniform throughout; the axial darkness being scarcely distinguishable, excepting on one occasion, July 3d.

The following positions have been derived from comparisons with neighboring stars.

1861.	Cambridge mean solar time.	α	δ
July 2,	$8^h\ 28^m\ 38^s$	$8^h\ 37^m\ 43^s.22$	$+62°\ 51'\ 17''.1$
3,	8 21 33	9 49 15 ·85	66 6 15 ·3
3,	10 39 52	9 56 6 ·58	66 16 05 ·1
4,	10 39 18	11 2 7 ·48	66 53 26 ·4
5,	12 9 26	11 57 9 ·67	66 3 22 ·0
6,	9 17 39	12 31 2 ·60	64 51 33 ·3
8,	10 20 5	13 21 36 ·05	61 46 13 ·7
9,	10 40 47	13 37 37 ·88	60 21 45 ·2
10,	9 39 12	13 49 26 ·80	59 9 34 ·1
12,	11 57 47	14 8 0 ·59	56 54 47 ·2
13,	9 47 55	14 13 59 ·24	56 5 25 ·7

The nucleus admitted of very precise observations; indeed it is a curious fact that it would be quite possible by means of proper comparisons with neighboring stars, to obtain the differences of terrestrial longitudes of the principal points at which

it was observed, with a degree of precision only surpassed by the more refined methods known in astronomy.

The near approach of the present comet to the earth and the sharply defined point of its nucleus, illustrates the practicability of a method of determining the solar parallax with perhaps greater exactness than can be attained by any other means. Many comets have stellar points for their nuclei, visible in the larger telescopes, which admit of as accurate comparisons with neighboring stars as is practicable in measurements among the stars themselves. Many such have appeared within the last fifteen years. Suppose such a comet to be suitably placed so as to be observed simultaneously in different quarters of the globe, when at a distance from the earth of less than one-twentieth of the sun's distance. Under favorable circumstances it would not be hazarding too much to say, that in the course of its apparition the probable error of the solar parallax could be reduced within smaller limits than is possible by means of transits of Venus or of any other method. Such an opportunity might possibly afford an improved value of the mass of the earth.

The following elements of the comet have been computed at the Observatory by Messrs. Safford and Hall.

Elements of Comet II, 1861. By T. H. Safford.

T = 1861, June 11.1878.		Cambridge m. t.
log. q =		9·91299
$\pi - \Omega = \omega$		329° 40′·81
Ω	=	278 59 ·28
i	=	85 41 ·43
		Motion direct.

By A. Hall.

T = June 11.280.		Washington m. t.
π	=	248° 51′ 50″
Ω	=	278 59 23
i	=	85 41 27
log. q =		9·91352
		Motion direct.

From the above, Mr. Safford finds the following approximate ephemeris for $10^h\ 24^m$ m. t. Washington, Δ and r representing the distances from the earth and sun respectively.

1861.	α	δ	log. Δ	log. r
July 5,	178° 42′	+66° 8′	9·3404	9·9716
6,	188 21	64 45		
7,	195 24	63 13	9·4281	9·9800
8,	200 37	61 42		
9,	204 34	60 18	9·5051	9·9887
10,	207 39	59 2		
11,	210 5	57 54	9·5722	9·9975
12,	212 5	56 52		
13,	213 44	+55 57	9·6313	0·0066

The following, computed by Mr. Hall for Washington mean midnight, will give an idea of the path of the comet previous to its becoming visible in the northern hemisphere.

W. m. t.	α	δ	log. Δ
June 12·5,	61° 39′·1	−26° 32′·3	9·7222
16·5,	63 16·3	23 18·4	9·6182
20·5,	66 4·9	17 11·6	9·4831
24·5,	71 56·9	− 3 45·4	9·3105
28·5,	85 17·9	+27 41·5	9·1420
July 2·5,	132 15·9	63 34·0	9·2027
6·5,	188 53·9	64 38·6	9·3884
10·5,	207 50·1	+58 56·5	9·5420
June 29·0,	89 30·4	+33 8·4	9·1330

The comet passed its ascending node June 28·086, Washington m. t., when the difference between the heliocentric longitude of the comet and of the earth was 2° 0′. The difference between the geocentric longitude of the comet and of the sun was 12° 29′. Log. of distance of the comet from the earth =9·1529. Calling its brilliancy unity on July 2·5, we have

June 12·5,	Brilliancy = 0·11
16·5,	0·18
20·5,	0·33
24·5,	0·69
28·5,	1·41
July 2·5,	1·00
6·5,	0·50
10·5,	0·18

computed by the equation, brilliancy $= \frac{1}{r^2 \Delta^2}$.

It will be seen that the comet, at about the time of its perihelion, must have been well situated for observation at the Cape of Good Hope and other points in southern latitudes. The *calculated* brilliancy is indeed much less than on the 2d of July, being only about one-tenth—but it is well known that the formula cannot be relied upon for the variations of the light of comets, which is greatly influenced by their positions relatively to their perihelia.

It is probable that at least the head of the comet was much brighter at the middle of June than it was after the first of July, and we shall wait with much interest for accounts of it from southern observatories, especially from the Cape of Good Hope, which has often, in similar emergencies, proved itself the most important astronomical position occupied by any existing observatory.

From the above elements, the diameter of the nucleus may be variously estimated at from one hundred and fifty to three or

four hundred miles. On July 2d the breadth of the head at the nucleus was 156,000 miles, the height of the inner envelope 11,500 miles, and the length of the tail about 15,000,000 miles.

The comet was seen between one and two o'clock on Sunday morning, June 30th, by Dr. Brünnow, at the Observatory of Ann Arbor. This is the earliest authentic account of its visibility which has come to my notice. The head could not have been seen on Friday evening, although observations to that effect have been reported. The extremity of the tail, however, must have been within view for some time previous, though too faint to attract notice.

The reports current of the identity of the comet with those of 1264 and 1556 are without any foundation.

4. *Continuation of Account of Comet II.* 1861; *by* G. P. Bond, *Director of the Observatory of Harvard College.*

With the exception of a single cloudy night, observations were made at this Observatory, on every evening from the 2d to the 21st July inclusive. A like succession of clear skies rarely occurs here. At the latter date, the comet had become much fainter, and was besides obscured by the moonlight. In the region near the nucleus, the outlines of the envelope could no longer be discerned —the distribution of the brighter portions of the nebulosity, however, could be seen to resemble that of the earlier jets. This peculiarity has been preserved up to the beginning of August.

The tail during the last days of July, as seen through an opera-glass or comet-seeker, has continued to show the narrow ray, now reduced to about 12° in length.

Near the head it forms the right hand side of the tail (as presented in the evening), which is the brightest and best defined. Further on, it seems to bend to the right hand with a decided curve; at least this was the impression to the eye, but it has become a very faint object. The hazy tail curving to the left, maintains its position and general form, but is greatly diminished in extent and brightness. Mr. Safford has computed the following elements from observations:

June 30th,	Greenwich.
July 8th and 9th,	Observatory of
July 18th.	Harvard College.

Elements of Comet II, 1861.

T =	June 11.5018.	Washington m. t.	
log. q =	9.915048		
$\pi - \Omega$ =	330°20′48″.1	M. Eq. 1861.0	
Ω =	278 58 40 .3		
i =	85 37 7 .0		

The observations of June 30th and July 18th were exactly represented, and those of July 8th and 9th as follows, by the method of least squares. A comparison upon Aug. 3 is also added.

	c − o			c − o	
	$\Delta\alpha$	$\Delta\delta$		$\Delta\alpha$	$\Delta\delta$
July 8th,	+ 1″·7	0″·0	Aug. 3d,	− 8″·7	+23″·1
9th,	− 4 ·0	−2 ·5			

The elements are somewhat closer approximations than those communicated in the previous notice, though not differing essentially from them.

The comet was observed extensively in Europe, on the evening of the 30th of June. The following passages are translated from a communication in No. 1316 of the *Astronomische Nachrichten*, from Prof. Secchi, Director of the Observatory of the Collegio Romano, dated Rome, July 1, 1861.

"Yesterday at 9^h 30^m, I noticed this immense train of light in the N.N.E. which I took for the smoke of the fireworks which they often have in the city on the festival of St. Peter and St. Paul, but I soon perceived that it was a comet. Its head was then so low that I could with difficulty take an approximate instrumental position, and at 9·39 m. t., I found its A. R. 6^h 37^m, and its dec. 45° 57′. After the head had set, the tail remained on the horizon, and was much more brilliant than the milky way.

About half an hour before midnight, the tail had reached its maximum development; it was very bright, and 6° broad at the pole star which was exactly in its centre. Thence it contracted considerably, the central part only remaining, which extended in a long band, tolerably bright, and about 2° broad, touching α Lyræ on one edge (the eastern), and passing the zenith, was lost in the milky way near the stars ζ and ε Aquilæ. Including the part below the horizon the length was 118°. This is truly surprising! After it had risen at 2 o'clock in the morning, I examined the details of the construction of the nucleus till about 3^h, and I found that the nucleus was elliptical, with its major axis situated transversely to the axis of the tail. The measure of this axis was 10″·05, and the minor axis was estimated as 2″ smaller, its light nearly equalling that of Saturn. It was well defined on all sides with a power of 200, only on the side towards the sun it was a little less sharp, while a great mass of jets of reddish light started from its edge in the form of a fan. The whole nucleus and the fan were surrounded with a halo, folding back in two branches to the right and left; the former longest and most brilliant; finally the whole was enveloped with a large, well defined, paraboloid segment. The distance from the apex of the paraboloid to the center of the nucleus was 3′ 12″, and that from the nucleus to the bend of the curved jets was 1′ 55″. The maximum breadth of the tail was about 6°." . . .

Dr. Pape of Altona has examined carefully the question whether the earth was at any time within the tail of the comet, and concludes that at its nearest approach we were about 2,400,000 miles distant from its nearest edge, on the morning of June 29th at $2^h\ 24^m$.

This result (so far as it relates to the principal ray) is also indicated by Mr. Hall's calculations communicated in the earlier notice. Mr. Hall states the difference of the heliocentric longitude of the comet and of the earth, at the passage of the node, June 28·086 Washington mean time, to be 2° 00′ and the log. distance of the comet from the earth 9·1529.

Dr. Pape finds for the nodal passage June 28·10 Washington mean time, difference of longitudes =1° 59′. The results in other particulars are also substantially the same.

The following remarks have been translated from Dr. Pape's article in No. 1316 of the *Astronomische Nachrichten*:

"From the position of the comet with respect to the sun and the earth, we might conjecture the latter to have passed through the tail of the comet on the 28th or 29th of last June. As such a collision is of some interest, I have undertaken a slight calculation to obtain closer details. The passage of the comet through its ascending node took place, Berlin time, June 28·35 (28·10 Washington).

At this time, its true anomaly was 29° 37′·5, log. r=9·94449, its heliocentric longitude 278° 59′, its distance from the earth's orbit =0·1161. At the same time the heliocentric longitude of the earth was 277° 0′. Assuming that the axis of the tail coincided with the prolongation of the radius vector, the earth was at a distance of 0·0352 from this axis. (Mean distance of the earth from the sun =1.)

A consideration of the dimensions of the tail shows that, according to the data, no collision took place.

That point of the tail which lay in the earth's orbit was at a distance of =0·1162 from the nucleus; to determine the diameter of the tail at that point, I subjoin an observation of the 3d of July.

At 10 o'clock on the evening of that day, the angle which the prolongation of the radius vector of the comet formed with the direction from the earth to the comet, was =57° 40′; a point in the tail (which could be traced for about 60 degrees) at a distance =0·1162, had an apparent distance of 42° 24′ from the nucleus, and at the same time that the distance of this point from the earth =0·1457.

At a distance of about 42° from the nucleus, the breadth of the tail may have been 3°; hence it follows that its true diameter was 0·0076.

Now on June 28·35, the distance of the earth from the axis of the tail was 0·0352, so that the nearest particles of the tail passed

by the earth at a distance of 0·031. This result however rests upon the supposition that the tail lay in the prolongation of the radius vector. In fact, it appears to have been curved in the plane of the orbit, so that the remotest particles of the tail passed through the plane of the ecliptic earlier than the nucleus. But in no case, did any collision take place. According to the above hypothesis, the least distance of the earth from the nearest of the tail particles was 0·025 at about 12 hours after the passage of the comet through the node."

Altona, July 9th, 1861.

The above calculations of Pape relate to the straight bright ray. There was, however, as will be seen by reference to the preceding part of this notice, a great mass of diffuse light which separated from the ray, and swept off towards *Corona Borealis* in the early part of July, reaching on the 4th to a distance of 40° from the nucleus, and having a breadth of 12° or 15°. This would have barely grazed the earth on the 30th of June. In a late number of the Bulletin of the Imperial Observatory at Paris, Mr. Hind suggests that a peculiar illumination of the sky noticed in England on that date was possibly "attributable to the commingling of the matter forming the tail of the comet with the earth's atmosphere." The observations on this day have a peculiar interest, from the fact that, at about 10^h mean time Greenwich, the earth passed the plane of the comet's orbit, and the outline of the tail presented to us was that of the section formed by a plane perpendicular to its orbit.

From its brilliancy and nearness to the earth, the opportunity of defining this section was perhaps the best which the history of astronomy has afforded. It is to be hoped that European astronomers, who it seems had the advantage of clear skies and a favorable position of the comet at the very hour of our crossing the orbit, have made good use of the occasion by recording carefully all peculiarities of the physical aspect.

Prof. Secchi has defined the position of the tail at $11^h\ 30^m$ m. t. at Rome, June 30, about 55^m after the earth's passage through the orbit plane. Comparing the position of an arc of a great circle drawn from the sun through the nucleus, which must have corresponded almost precisely with the projection of the plane of the orbit, we find that at the pole star the centre of the ray was about $1\frac{1}{8}°$ to the *east* of the circle.

At α Lyræ the eastern edge was at the same distance to the west of the circle, so that the entire breadth was on that side; the centre being distant

$$1\tfrac{1}{8}° + \tfrac{1}{2} \text{ (breadth of tail).}$$

Again at ζ and ε Aquilæ the tail was about 4° to the east of the circle. There was therefore a decided *bend* in the tail

which on account of the direction of the time of vision must have been *out of the plane of the orbit.* That its entire breadth should have been out of this plane, as it seems to have been near α Lyræ, is indeed a most remarkable feature and very important in its bearing upon the theory of the forces concerned in producing it. At later dates, as we have remarked in a previous notice, the tortuous path of the ray was obvious.

Cambridge, Mass., Aug. 3, 1861.

Cambridge, August 8th.

P. S.—A letter of the date June 17, received from Dr. Moesta of the National Observatory, Santiago, Chili, announces the appearance of "a brilliant comet" early in the month. Its position and motion agree with the ephemeris communicated in a former part of this notice. We extract the following observations:

	Mean time Santiago.	α	δ
1861, June 10,	$18^h\ 18^m\ 26^s{\cdot}1$	$4^h\ 3^m\ 52^s{\cdot}92$	$-27°\ 51'\ 8''{\cdot}8$
12,	17 46 16 ·4	4 6 5 ·17	−26 47 14 ·7

Cambridge, August 10th.

Mr. Safford's latest elements represent the Santiago observations as follows:—

	comp.—obs. $\Delta\alpha$	$\Delta\delta$
1861, June 10,	$-36''{\cdot}0$	$+22''{\cdot}9$
12,	−15 ·7	−16 ·6

There can not therefore be any considerable deviation of the orbit from a parabola.

The tail of the comet was seen 40° long by Liais at Rio Janeiro on the 14th of June.

5. *Additional Observations at the U. S. Naval Observatory, Washington.* Communicated by Lt. J. M. Gilliss, U. S. N.*

Upon attempting to compute an orbit for the comet, Prof. Hubbard found it impossible to represent the observed path by a parabola, and then obtained by the Gaussian method the following hyperbolic elements, based upon the places obtained by Mr. Ferguson on the 2d, 8th, and 17th of July. They give for the middle date the values $\Delta l = -0''{\cdot}33$, $\Delta b = -0''{\cdot}16$.

Time of perihelion passage,	1861, June 11.85294.	Wash. M. T.
Long. of perihelion,	249° 44′ 44″·58	M. equinox,
" " node,	278 59 49 ·72	1861·0
Inclination,	85 56 8 ·86	
Excentricity,	1·0265470	
Perihelion distance,	0·7453901	

* From Lt. Gilliss' paper, to appear in full in our next issue.

The following is the list of observations, as far as reduced and compared with the above elements:

Wash. M. T.	α	c.—o.	δ	c.—o.
d	° ′ ″	″	° ′ ″	″
July 2·41250	130 48 6·8	+ 2·4	63 12 48·5	+ 1·3
3·36480	147 56 52·0	+17·9	66 10 2·4	− 0·6
·38102	148 14 43·1	− 9·5	66 11 49·9	− 4·7
·63908	152 46 48·8	+24·8	66 34 17·6	− 2·5
4·36786	164 40 26·8	+18·6	66 54 21·2	− 5·5
6·37421	187 49 0·3	+ 2·5	64 51 1·7	− 5·8
8·36769	200 12 23·3	− 0·5	61 50 2·0	− 0·8
10·53104	207 46 0·2	− 1·4	58 58 55·8	− 9·4
11·45260	209 59 3·0	+ 2·3	57 56 58·2	−10·2
12·42192	211 54 9·1	+ 8·9	56 58 12·1	− 7·8
14·43062	214 56 37·2	+ 7·5	55 14 25·5	− 6·1
16·37969	217 6 9·6	−13·4	53 52 5·4	+ 5·2
17·35071	217 58 33·0	− 4·8	53 16 37·0	+ 1·5
·42154	218 1 58·8	+ 4·2	53 14 13·7	− 3·8
20·40982	220 9 54·0	+16·7	51 42 28·5	− 3·8
23·37224	221 44 40·7	+19·5	50 30 17·2	− 9·1
24·35193	222 11 27·8	+28·1	50 9 26·8	− 9·8
25·36437	222 37 26·0	+32·5	49 49 15·3	−13·2
27·37080	223 24 38·3	+36·0	49 12 25·8	− 8·9

From these residuals it is evident that the orbit requires yet some correction, not sufficient, however, it is believed, to change its decided hyperbolic nature.

6. *Elements of the Comet, in a letter to the Editors*, by Mr. M. C. Stevens.

I took the position of the late great comet on the 4th, 8th and 12th ult., at our Observatory with the following results:

Mean time of Observation.	Geocentric Long. of	Geocentric Lat. of
	° ′ ″	° ′ ″
July 4.39260	129 31 32	53 25 51
" 8.40480	155 48 12	60 55 24
" 12.37426	171 31 42	62 7 7

From these I have computed the following elements of its orbit:—

Long. of ascending node,	° ′ ″ 278 57 52
Inclination of orbit,	85 38 7·5
Long. of perihelion,	248 30 31
Perihelion distance,	Log. 9·91248
Perihelion passage, 1861, June 11.12177. Motion direct.	Washington M. T.

Haverford Observatory, West Haverford, Pa., 8th mo. 15th, 1861.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY.

TECHNICAL CHEMISTRY.—

5. *On the Coloring Matters Derived from Coal-Tar;* by Mr. H. W. PERKIN, F.C.S. (A lecture delivered before the [London] Chemical Society on Thursday, May 16, 1861); from the [London] Journal of Gas Lighting, etc. p. 447, June 18, 1861.—The subject which I have the honor of bringing before you this evening is one which is remarkable for the very rapid progress it is making. Previous to the year 1856, the coloring matters derived from coal-tar products were practically unknown; but, owing to recent discoveries, they are now enumerated among the most important coloring matters employed. It is not my intention to enter into a discussion respecting the numerous patents that have been taken out for the prodution and application of coal-tar colors, nor yet to enter into the practical details of their manufacture; but to notice more particularly their various chemical characters, and also to speak of the methods employed for their application to the arts. I shall endeavor to bring before you all the principal colored products obtainable from the derivatives of coal-tar—both those which are practically employed by the dyer and printer, as well as those which still remain as chemical curiosities only. I may mention that the latter class of compounds is more extensive than the former, and also that many of the coloring matters in it are likely to remain so, for reasons to be mentioned presently.

The coal-tar products which have, up to the present, yielded coloring matters, are numerous. The following is a list of the most important, which are obtained either directly or indirectly from coal-tar:—namely, aniline and its homologues, carbolic acid, chinoline or quinoline, naphthaline, and pyrole bases.

I will commence by speaking of the first and most important of these products, and its derivatives—namely, Aniline.

Aniline was discovered in 1826 by Unverdorben. The original method for its preparation was by digesting indigo with hydrate of potassa, and subjecting the resulting product to distillation. Aniline was also obtained from the basic oils of coal-tar; but the process which is now employed for its preparation is a remarkable instance of the manner in which abstract scientific research becomes, in the course of time, of the most important practical service. It was Faraday who first discovered benzole; he found it in oil-gas. After this, it was obtained by distilling benzoic acid with baryta, which result determined its formula, and was the cause of it being called *benzole*. After this, Mansfield found it to exist in large quantities in common coal-tar naphtha, which is the source from which it is now obtained in very large quantities. Benzole, when studied in the laboratory, was found to yield, under the influence of nitric acid, nitrobenzole. Zinin afterwards discovered the remarkable reaction which sulphid of ammonium exerts upon nitrobenzole, converting it into aniline. And, lastly, Béchamp found that nitrobenzole was converted into aniline when submitted to the action of ferrous acetate. It is Béchamp's process which is now employed for the preparation of aniline by the ton. Had

it not been for the investigations cited above, the beautiful aniline colors now so extensively employed, would still remain unknown. When I discovered aniline-purple, nitrobenzole and aniline were only to be met with in the laboratory; in fact, half a pound of aniline was then esteemed quite a treasure, and it was not until a great deal of time and money had been expended that I succeeded in obtaining this substance in large quantities, and at a price sufficiently low for commercial purposes.

The coloring matters obtainable from aniline are numerous. They are the following:—Aniline purple, violine, roseine, fuchsine, alpha aniline purple, bleu de Paris, nitroso-phenyline, dinitraniline, and nitro-phenyline diamine.

Aniline Purple, generally known as Tyrian Purple, Mauve, Phenamine, Indisine, &c.—It has been known for many years that the hypochlorites react on aniline and its salts, producing a purple-colored solution; in fact, hypochlorites are the distinguishing test for aniline; but nothing definite was known of this purple-colored solution, it being simply stated that aniline produced, with hypochlorites, a purple-colored liquid, but that this color was very fugitive. As many very absurd statements have been made respecting the discovery of aniline purple, I will just briefly mention how it was that I first became acquainted with it.

In the early part of 1856, I commenced an investigation on the artificial formation of quinine. To obtain this base, I proposed to act on toluidine with iodid of allyle, so as to form allyle-toluidine, which has the formula—

$$\left.\begin{matrix} C_7H_7 \\ C_3H_5 \\ H \end{matrix}\right\} N = C_{10}H_{13}N,$$

thinking it not improbable that, by oxydizing this, I might obtain the desired result; thus—

$$\underbrace{2(C_{10}H_{13}N)+O_2}_{\text{Allyle-toluidine.}} = \underbrace{C_{20}H_{24}N_2O_2+H_2O}_{\text{Quinine.}}.$$

For this purpose, I mixed the neutral sulphate of allyle-toluidine with bichromate of potassa; but, instead of quinine, I obtained a dirty reddish-brown precipitate. Nevertheless, being anxious to know more about this curious reaction, I proceeded to examine a more simple base under the same circumstances. For this purpose, I selected aniline, and treated its sulphate with bichromate of potassa. This mixture produced nothing but a very unpromising black precipitate; but, on investigating this precipitate, I found it to contain the substance which is now, I may say, a commercial necessity—namely, aniline purple.

The method adopted for the preparation of analine purple is as follows:—Solutions of equivalent proportions of sulphate of aniline and bichromate of potassa are mixed, and allowed to stand till the reaction is complete. The resulting black precipitate is then thrown on a filter, and washed with water until free from sulphate of potassa. It is then dried. This dry product is afterwards digested several times with coal-tar naphtha, until all resinous matter is separated, and the naphtha ceases to be colored brown. After this, it is repeatedly boiled with alcohol to extract the coloring matter. This alcoholic solution, when distilled, leaves the col-

oring matter in the bottom of the retort as a beautiful bronze-colored substance.

The aniline purple prepared according to the process just described, although suitable for practical purposes, is not chemically pure. If required pure, it is best to boil it in a large quantity of water, then filter the resulting colored solution, and precipitate the coloring matter from it by means of an alkali. The precipitate thus obtained should be collected on a filter washed with water until free from alkali, and dried. When dry, it is to be dissolved in absolute alcohol, the resulting solution filtered, and then evaporated to dryness over the water-bath. Thus obtained, aniline purple appears as a brittle substance, having a beautiful bronze-colored surface; but, if some of its alcoholic solution be evaporated on a glass plate, and viewed by transmitted light, it appears a beautiful bluish violet color. If considerable quantities of an alcoholic solution of the coloring matter, containing a little water be evaporated to dryness, the surface of the coloring matter next to the evaporating dish, when detached, often possesses a golden-green appearance. Aniline purple is difficultly soluble in cold water, although it imparts a deep purple color to that liquid. It is more soluble in hot water, but its hot aqueous solution, when left to cool, assumes the form of a purple jelly. It is very soluble in alcohols, though nearly insoluble in ether and hydrocarbons. Aniline dissolves it readily. In properties it seems to be slightly basic, as it is more soluble in acidulated than in pure water. Alkalies and saline substances precipitate it from its aqueous solution as a dark purplish-black powder. Bichlorid of mercury precipitates it in a very finely-divided state. A little of this precipitate, which appears to be a double compound of chlorid of mercury and coloring matter, when suspended in water and viewed by transmitted light, appears of a blue or violet color. If a small quantity of hydrate of potassium or sodium be added to an alcoholic solution of the coloring matter, it causes it to assume a violet tint, but without effecting any change in the coloring matter itself. Ebullition with alcoholic potash does not decompose it. Aniline purple dissolves in concentrated sulphuric acid, forming a dirty-green solution. This, when slightly diluted, assumes a beautiful blue color. Excess of water restores it to its original purple color. I have had a specimen of this coloring matter heated for an hour to 100° centigrade, with Nordhausen sulphuric acid without suffering decomposition, being restored to its original color by means of water, and possessing precisely the same properties as it had before being subjected to this powerful agent. Hydrochloric acid acts upon it in the same manner as sulphuric acid. It is decomposed by chlorine, and also by fuming nitric acid. Bichlorid of tin is without action upon it. Powerful reducing agents have a peculiar action upon this coloring matter, somewhat analagous to the action of reducing agents on indigo. An alcoholic solution of sulphid of ammonium, when mixed with an alcoholic solution of the coloring matter, causes it to assume a pale, slightly brownish color. This solution, when brought in contact with the atmosphere, instantly assumes its beauty and intensity of color. An alcoholic solution of the coloring matter, when mixed with a little protoxyd of iron, changes to a pale-brown color. This solution also becomes purple when exposed to the action of the atmosphere. Sulphurous acid does not affect the color of this substance.

The coloring matter forms a remarkable compound with tannin. When an aqueous solution of the coloring matter is mixed with a solution of the tannin, precipitation takes place. The precipitate thus formed, after having been well washed, no longer possesses the properties of the pure coloring matter. It is insoluble in water. Like the pure coloring matter, it dissolves in concentrated sulphuric acid, forming a dirty-green liquid; but, on adding an excess of water to this solution, the new compound is precipitated unchanged. This compound is rather duller in color than the pure coloring matter itself. Aniline purple, when agitated with a little moist binoxyd of lead, is transformed into roseine. This coloring matter is remarkable for its intensity—a few grains of it coloring a considerable quantity of spirits of wine.

Violine.—This coloring matter, which is a product of the oxydation of aniline, was first obtained by Dr. David Price. He prepared it by heating an aqueous liquid containing two equivalents of sulphuric acid, and one equivalent of aniline, to the boiling-point, and then adding one equivalent of binoxyd of lead, boiling the mixture for some time, and filtering it whilst hot. The filtrate, which is of a dark purple hue, is boiled with potash, both to separate the excess of aniline, and also to precipitate the coloring matter. When all the free aniline is volatilized, the residue is thrown on a filter, and slightly washed with water, and then dissolved in a dilute solution of tartaric acid. This solution after filtration is evaporated to a small bulk, refiltered, and then precipitated by means of an alkali. Thus obtained, violine presents itself as a blackish-purple powder, which, when dissolved in alcohol, and evaporated to dryness, appears as a brittle, bronze-colored substance, similar to aniline purple, but possessing a more coppery-colored reflection. It is more insoluble in water than the preceding coloring matter; it is very soluble in alcohol, but insoluble in ether and hydrocarbons. These solutions possess a color somewhat similar to that of the field violet. Concentrated sulphuric acid dissolves it, forming a green solution, but excess of water restores it to its original color. Like aniline purple, reducing agents deprive it of its color, but which is restored by the action of the atmosphere. Tannin produces an insoluble compound with it; when agitated with a small quantity of binoxyd of lead, it is converted into aniline purple; excess of this reagent changes it into roseine.

Roseine.—This substance nearly always accompanies aniline purple, though in very small quantities. It was first noticed publicly by C. Greville Williams, and afterward by Dr. David Price. Williams used manganates for its preparation, but Dr. David Price prepared it by means of binoxyd of lead. His process is as follows:—To a boiling solution of one equivalent of sulphate of aniline, two equivalents of binoxyd of lead are added, and the mixture boiled for a short time. The rose-colored solution is then filtered, and the filtrate evaporated to a small bulk, which causes a certain amount of resinous matter to be separated. This evaporated solution is then filtered, and the coloring matter precipitated by means of an alkali; it is then collected on a filter, slightly washed, and then dried. The coloring matter thus prepared readily dissolves in alcohol, forming a fine crimson-colored liquid, which, when evaporated to dryness, leaves the coloring matter as a dark brittle substance, having a slightly metallic reflection. It is much more soluble in water than

either aniline purple or violine; but, like them, it is insoluble in hydrocarbons, and is more soluble in acids than in neutral liquids. Concentrated sulphuric acid dissolves it, forming a green solution; excess of water restores it to its original color. It forms a compound with tannin, and is also deodorized, or nearly so, by powerful reducing agents.

The three coloring matters just mentioned, namely, aniline purple, violine, and roseine, are evidently closely allied, for they have nearly the same properties. They are all formed under similar circumstances, namely, by the action of oxydizing agents in the presence of water; they are all slightly soluble in water, though as the shade of color becomes redder, so their solubility increases; alkalies precipitate them from their aqueous solutions; concentrated sulphuric acid dissolves them, forming green solutions, which excess of water restores to the original color of the coloring matters; powerful reducing agents deprive them of their color, or nearly so; but it is again restored by the influence of oxygen; and, lastly, tannin forms insoluble compounds with them all.

Fuchsine or Magenta.—This beautiful product, which is often improperly called roseine, is a member of an entirely different series of compounds from the foregoing, being formed under very different circumstances, and possessing very different properties. This coloring matter was first observed by Natanson, in 1856, when studying the action of chlorid of ethylene on aniline, and afterwards, shortly before it was practically introduced into the arts, by Dr. Hofmann, when preparing cyantriphenyle-diamine by the action of bichlorid of carbon on aniline. It was Mr. Verguin who first brought it forward as a dyeing agent, and who, I believe, taught manufacturers how to prepare it on the large scale. Fuchsine is invariably formed at a temperature ranging from 170° to 190° centigrade. It is produced from aniline by the action of reducible chlorinized, brominized, iodized, or fluorized substances, as well as by weak oxydizing agents. The substances used for its preparation on the large scale are perchlorids of tin and of mercury, and the nitrates of mercury. It has also been prepared with bichlorid of carbon.

Preparation of Fuchsine by the Action of Bichlorid of Tin on Aniline.—Aniline combines with bichlorid of tin, evidently producing a double compound. This product is a white substance, and may be prepared by adding to aniline, bichlorid of tin in the anhydrous state, or dissolved in water. Anhydrous bichlorid of tin combines with aniline with great energy to form this compound. To prepare fuchsine from this double compound, it is necessary that it should be free from water, or nearly so; therefore, anhydrous chlorid of tin is generally employed for its preparation. The process adopted is as follows:—Anhydrous bichlorid of tin is slowly added to an excess of aniline, the mixture being constantly stirred, and the pasty mass thus formed gradually heated. As the temperature increases, it becomes quite liquid and also brown in color. As soon as the temperature nearly approaches the boiling point of the mixture, it rapidly changes to a black-looking liquid which, when viewed in thin layers, presents a rich crimson color. This is kept at its boiling point some time, and then well boiled with a large quantity of water. By this means, the principal part of the coloring matter is extracted, together with considerable quantities of hydrochlorid of aniline. The residue is a solid of brown color, and contains considerable quantities of

tin in the form of a proto-compound. The aqueous solution of the coloring matter and hydrochlorate of aniline is then boiled, so as to volatilize any free aniline it may contain, and then saturated with chlorid of sodium. The chlorid of sodium causes the coloring matter to separate as a semi-solid pitchy substance, of a golden-green aspect, while the hydrochlorate of aniline remains in solution. The coloring matter thus obtained may be further purified by digestion with benzole, which dissolves out a certain amount of resinous matter.

Preparation of Fuchsine by the Action of Nitrate of Mercury on Aniline.—When protonitrate of mercury is left in contact with aniline for some time it forms a white pasty mass; but when carefully heated to 170° or 180° centigrade, it reacts upon it, forming a brown liquid, which gradually changes till of a dark crimson color. At the same time, the whole of the metal of the mercury-salt collects at the bottom of the vessel the experiment is conducted in. This product, when separated from the metallic mercury and allowed to cool, becomes semi-solid, being filled with crystals of nitrate of aniline. To purify this product, it is best to dissolve out the nitrate of aniline it contains in a small quantity of cold water, and then to boil the remaining product several times with fresh quantities of water until the principal of the coloring matter is extracted, and filter the resulting aqueous solutions while hot. On cooling, the solutions will deposit the coloring matter as a golden-green tarry substance, from which benzole separates a small quantity of a brown impurity, leaving the coloring matter as a brittle solid.

I have briefly described the above processes, because they may to some extent be regarded as types of most of the methods employed for the production of this coloring matter—the first representing its formation by the action of reducible chlorids upon aniline, and the latter by the influence of weak oxydizing agents.

Fuchsine is undoubtedly an organic base, and a more powerful one than is generally supposed. The product obtained from aniline by means of bichlorid of tin is hydrochlorate of fuchsine, and that obtained by the oxydizing action of nitrate of mercury is the nitrate of fuchsine. My reason for stating this is, that, on examining the coloring matter obtained by chlorid of tin, I found it to contain large quantities of combined hydrochloric acid, and, when nitrate of mercury was used, considerable quantities of combined nitric acid; therefore, I concluded that the former is the hydrochlorate, and the latter the nitrate.

Fuchsine is separated from its salts by precipitation with a small quantity of ammonia. When freshly precipitated, fuchsine is a red bulky paste, which, when dry, contracts, forming a purplish red powder. It is difficultly soluble in water, though tolerably soluble in alcohol; it is not soluble in ether or hydrocarbons; a small quantity of hydrochloric acid causes it to dissolve freely in hot water; but an excess of either hydrochloric acid or sulphuric acid dissolves it, forming a brownish yellow liquid from which ammonia separates it unchanged. By this reaction it may be distinguished from roseine, which dissolves in strong sulphuric acid, producing a green liquid. Caustic alkalies, or ammonia in excess, partially precipitate fuchsine from its salts; but, at the same time, dissolve a considerable quantity of it, forming nearly colorless liquids.

Acetic acid added to these alkaline solutions restores the color of the fuchsine; and, if the liquids are concentrated, the base precipitates as a red, flocculent substance. An alcoholic solution of fuchsine, when evaporated to dryness, leaves the coloring matter as a brittle mass, having a remarkably beautiful golden-green metallic reflection. By transmitted light it has a red color. Mr. Béchamp has analyzed carefully-prepared specimens of fuchsine, and found it to have the formula $C_{12}H_{12}N_2O$. This hydrochlorate he found to contain an amount of hydrochloric acid, corresponding with the formula $C_{12}H_{12}N_2O$, H Cl. He also examined the hydrochloroplatinate, which is a purple precipitate; it has the formula $C_{12}H_{12}N_2O$ H Pt Cl_3. The existence of oxygen in this base is remarkable, because, in many instances, it is produced from agents which do not contain a trace of oxygen—as, for example, bichlorid of tin and aniline. The only way I can account for the presence of oxygen in the product analyzed is, that it was a hydrate; thus:—

$$\underbrace{C_{12}H_{12}N_2O}_{\text{Béchamp's fuchsine.}} = \underbrace{C_{12}H_{10}N_2}_{\text{Anhydrous fuchsine.}} + \underbrace{H_2O}_{\text{Water.}}$$

This is, perhaps, to some extent, confirmed by an experiment I made with iodaniline. I find that iodaniline, when heated, yields fuchsine. This change can only be expressed thus:—

$$\underbrace{2(C_6[H_6I]N)}_{\text{Iodaniline.}} = \underbrace{C_{12}H_{10}N_2}_{\text{Anhydrous fuchsine.}} + 2HI$$

But, supposing the fuchsine examined by Mr. Béchamp to have been a hydrate, it is remarkable that its hydrochlorate, and, more particularly, its hydrochlorplatinate, should also be hydrates. But, as our knowledge of this body is as yet but scanty, we must wait for the accumulation of facts before we can form any fixed opinion respecting its constitution. The compounds investigated by Mr. Béchamp appear to be uncrystallizable. Some time back, I had upwards of 100 gallons of a hot, aqueous solution of this coloring matter, which had been prepared by means of nitrate of mercury. This solution, on standing until cold, deposited a considerable quantity of coloring matter in the form of small octahedra, having a most beautiful green metallic reflection. Reducing agents decolorize fuchsine, but the oxygen of the atmosphere restores it to its original color. If an alcoholic solution of fuchsine be left in contact with sulphid of ammonium until decolorized, or nearly so, and then exposed to the atmosphere, its color is immediately restored; but, if left to stand for several days, and then submitted to the atmosphere, several hours elapse before it assumes its original color. Like aniline purple, fuchsine is a very intense coloring matter. Tannin precipitates both fuchsine and its salts, forming difficultly soluble substances. Bichlorid of mercury also precipitates this base and its salts, forming double compounds. When preparing fuchsine by means of bichlorid of tin, or more particularly when using nitrate of mercury, there are two other coloring matters produced, one possessing an orange color, and the other a purple hue. It is on account of the presence of the former that some samples of commercial fuchsine possess a scarlet character of color. Of this

orange-coloring matter, scarcely anything is known. It is not precipitated from these salts by means of alkalies. Of the purple-coloring matter, little is also known. It is formed at the same time as fuchsine, but in very small quantities, and from which it is difficult to separate perfectly. Its properties are similar to those of fuchsine. It dissolves in concentrated sulphuric acid, forming a reddish-brown solution, from which part of the coloring matter is separated by dilution with water. Hydrochloric acid dissolves it, forming a dirty, yellowish-red liquid.—(*To be continued.*)

2. *Action of Sulphate of Copper when employed as a preservative of wood.* —Kœnig has investigated the chemical reactions which occur when wood is impregnated with a preservative solution of blue vitriol. He finds as a general rule, that a certain quantity of basic sulphate of copper remains combined in the pores of the wood in such a manner that it cannot be washed out with water. The copper salt may be seen by its green color in the spaces between the yearly rings in the less compact portions of the wood, that is to say in those portions which contain the sap. Those varieties of wood which contain the most resin retain the largest amount of the copper salt,—oak, for example, retaining but little of it. The ligneous fibre itself appears to have little or nothing to do with the fixation of the copper salt, and indeed none whatever is retained in chemical combination, so that it cannot be washed out with water, by pure cellulose. When wood from which all resin has been extracted by boiling alcohol, is impregnated with sulphate of copper, it does not become colored like the original resinous wood, and the copper salt contained in it may readily be washed out with water. In like manner, from impregnated resinous wood all the copper salt may be removed, with the resin, by means of alcohol.

The constituents of the blue vitriol are consequently fixed in the wood by means of the resin which this contains.

Further it is found that the impregnated wood contains less nitrogen than that which is unimpregnated and that it is even possible to remove all the nitrogenous components of the wood by long continued treatment with the solution of sulphate of copper. The nitrogenous matters being soluble in an excess of this solution just as the precipitate which forms when aqueous solutions of albumen and sulphate of copper are mixed is soluble in excess of the latter. Since the nitrogenous matters are well known to be promoters of putrefaction, their removal readily accounts for the increased durability of the impregnated wood.

The author hopes to explain in a similar manner the action of other salts, like chlorid of zinc, &c., which are used for preserving timber, and is now engaged in investigating the question.

The utility of blue vitriol as a preservative may also depend in a measure upon the resiuous copper salt which is formed, by which the pores of the wood are more or less filled up and the ligneous fibre covered so that contact with the air is prevented and the attacks of insects hindered. It is suggested that those cases in which the anticipated benefits have not been realized in practice by impregnating wood with a solution of blue vitriol, may probably be referred to the use of an insufficient amount of this agent—i. e., where the wood was not immersed in the solution for a sufficient length of time. The action should be one of lixiviation, not

merely of absorption.—*Programm der Realschule zu Leipzig*, 1861; Bœttger's *polyt. Notizblatt*, xvi, 177, (1861, No. 12).

3. *Ozone as a means of restoring old and faded engravings, etc.*—According to v. GORUP-BESANEZ, ozone when properly applied is a most effective and convenient agent for restoring books or prints which have become brown by age, or been soiled or smeared with coloring matter; only a short time being required to render them perfectly white, as if just from the press, and this without injuring in the least the blackness of the printer's ink or the lines of crayon drawings.

As examples of his results the author mentions a book of the sixteenth century upon a page of which several sentences had been painted over, by the monks of that epoch, with a black, shining coloring matter in order to render them illegible, and of which no trace of a line could be detected. After 36 hours treatment with ozone the coloring matter was entirely destroyed, and the most careful scrutiny of the page would have failed to discover that any of the lines had once been painted over. In like manner a wood cut of Durer which had been besmeared with a dark yellow color was completely restored.

Writing ink may be readily discharged by ozone, especially if the paper be subsequently treated with very dilute chlorhydric acid to remove the oxyd of iron.

Printer's ink is not attacked by ozone to any appreciable extent unless the action be long continued. Vegetable coloring matters are completely removed by it, but metallic coloring matters, grease spots and stains produced by fungi cannot thus be destroyed.

As applied in the small way, the method consists in placing a bit of phosphorus about 3 inches in length and ½ an inch in diameter, the surface of which has been scraped bright, in a wide necked glass carboy, or other large hollow vessel, pouring in as much water, at about 30° (C.), as will half cover the phosphorus, closing the vessel with a cork, and allowing the whole to stand until the jar is charged as strongly as possible with ozone, which ordinarily occurs after 12 or 18 hours. Then without removing the phosphorus or water, the paper to be bleached, which has been moistened with water, rolled up, and fastened to a platiuum wire in a suitable manner, is hung in the middle of the vessel. The cork is now restored and the apparatus left to itself. The roll of paper is soon surrounded with the fumes arising from the phosphorus and the stains gradually disappear. The rapidity of the operation of course depends upon the nature of the substance to be discharged—three days having been the longest time required in any of the experiments. Prints which had merely become brown by age and those stained with coffee usually become perfectly white and clean in the course of 48 hours. The action of the ozone however must not be continued too long lest some of the finer lines of the engraving should be injured. After all the spots have disappeared, the paper is strongly acid and if allowed to dry when in this condition would become exceedingly brittle and also dark colored. It is consequently necessary to remove the acid completely. In order to accomplish this the paper is placed in water which is frequently renewed and allowed to lie there until a bit of blue litmus paper pressed against it is no longer reddened. The paper is then passed through water to

which a few drops of a solution of soda have been added and is spread upon a glass plate, this is slightly inclined and a fine stream of water is allowed to flow over the paper during 24 hours. After the paper, on exposure to the air, has become dry enough to remove from the glass without danger of tearing, it is taken off and pressed dry between folds of filter paper.

The author remarks that in case the process were attempted on a larger scale it would probably be well to have glass troughs or boxes blown of the desired form, since it is not easy to prepare suitable vessels by any process of fastening together pieces of glass, the cement being attacked by ozone.

Attempts to apply ozone in restoring oil paintings gave only negative results, the action having been irregular.—*Annalen der Chemie und Pharmacie*, cxviii, 232.

4. *New Anæsthetic.*—During the past few months considerable interest has been excited among members of the medical profession by an attempt to introduce into practice a volatile liquid possessing anæsthetic properties which is obtained as an incidental product in the manufacture of coal oil. Of the chemical history of this substance—called keroselene by its manufacturers, but little is as yet known. Prof. Bacon, of the Harvard Medical School, informs us "that a sample in his possession is of sp. gr. 0·640, at 72° F. When heated in a flask containing scraps of platinum foil it began to boil at about 85° F. As the more volatile parts distilled off, the temperature continued to rise, and at 170° about three-quarters of the liquid had evaporated. It continued to boil feebly, but the whole was not converted into vapor until the thermometer had risen considerably above 300°; and when the flask was allowed to cool, much of the vapor condensed before the temperature had fallen to 300°. It is evident that several, perhaps many, hydrocarbons are present, having a wide range of boiling points. Probably the most volatile of them would be gaseous at ordinary temperatures, if isolated. It is remarkable that the keroselene should be so readily and completely volatile at atmospheric temperatures. I found that keroselene and Squibb's ether, exposed in watch glasses, lost equal weights in $2\frac{1}{2}$ and $3\frac{1}{2}$ minutes respectively; and the former evaporated completely in about two-thirds of the time required for the ether. The specimen which I examined contained a little *sulphur*. Some sulphur compound was therefore present as an impurity, which would be decidedly objectionable for anæsthetic purposes."

The vapor of this substance possesses very decided anæsthetic properties. This was first accidentally noticed by its effects upon a laborer engaged in cleaning a cistern at a coal-oil manufactory, and afterwards proved by the workmen by experiments upon flies and mice. Whether it can be employed without danger as a substitute for ether or chloroform is as yet undecided. Dr. H. J. Bigelow (Boston Medical and Surgical Journal (July 11, 1861) lxiv, 494,) reports several cases in which its exhibition was attended with unfavorable symptoms; and at the present time the general feeling of medical men in Boston with regard to its value is evidently much less favorable than when it was first brought forward. It may be remarked that the "keroselene" in question is

exceedingly well purified as far as relates to its odor, being almost entirely free from the objectionable smell which characterizes most of the light coal oils. F. H. S.

Boston, Aug. 20.

ANALYTICAL CHEMISTRY.—

5. *Reinsch's test for Arsenic not applicable to solutions containing Arsenic acid.*—WERTHER finds that the practice of applying Reinsch's test immediately to the liquid obtained by the action of chlorate of potash and chlorhydric acid upon the substance suspected to contain arsenic must be discarded and that, as a general rule, the test is inapplicable to liquids which contain arsenic acid.

It had heretofore been supposed, and the belief is countenanced by so high an authority as Fresenius (*Anleitung zur qualitativen chem. Analyse*, 10^te^ *Aufl.*, p. 148, §132, No. 7), that arsenic acid would readily be reduced by the copper employed in the test and that the products of this reduction would not interfere with the deposition of the characteristic arsenical coating upon the copper. But Werther now shows that solutions either of free arsenic acids, or of its salts in chlorhydric or sulphuric acid deposit no coating upon bright metallic copper unless a considerable quantity of arsenic acid be present. Similar negative results were obtained from solutions which were allowed to stand upon the copper in the cold for months and from others which were boiled at longer or shorter intervals.

It is consequently with arsenious acid alone that the characteristic reaction of arsenic can be exhibited when dilute solutions of the latter are boiled with copper and chlorhydric acid.

In the experiments of the author no unequivocal, lustrous, arsenical coating upon the copper could be obtained, on boiling for a few minutes, from acid solutions of arseniate of magnesia and ammonia unless these contained at least 0·4 à 0·5 grm. of arsenic in 50 cc.; although dark colored coatings of uncertain composition, from which no arsenic could be obtained by volatilization in a current of hydrogen or of carbonic acid, were obtained from more dilute solutions.

In judicial investigations, therefore, Reinsch's test should never be employed upon the liquid resulting from the oxydation of organic substances, until the arsenic acid therein contained has been reduced by a current of sulphurous acid.

Having observed that the dubious black coating sometimes obtained from dilute solutions of arsenic acid, as just described, frequently disappeared again as he sought to increase it by longer continued action, the author was led to the observation that not only the coating in question but pure metallic arsenic is gradually dissolved by chlorid of copper when this is gently heated. After having acted during a few hours the solution contains considerable quantities of arsenic acid but scarcely a trace of arsenious acid.

On the other hand common chlorhydric acid diluted with an equal volume of water being boiled upon bright metallic arsenic dissolved none of it, nor was any dissolved after digesting for two days at 50° à 60° (C.) in a closed vessel.—*Journal für praktische Chemie*, lxxxii, 286.

II. GEOLOGY.

1. *Reply to Mr. Marcou's strictures on Mr. F. B. Meek in his Notes on the Cretaceous and Carboniferous Rocks of Texas*, (in a letter to the Editors.)—GENTLEMEN:—I had intended never to answer any of Mr. Marcou's papers on the disputed points in the geology of the far-west, excepting at such times as I could do so in connection with the announcement of newly discovered facts. To prolong a discussion of this kind by merely reiterating opinions already expressed, after all the facts in possession of the parties have been published, can neither advance the interests of science, nor prove very interesting to scientific readers. In a paper recently published by Mr. Marcou, however, he makes some statements of a personal nature, which my friends think, if allowed to pass unnoticed, may place me in a false position.

The paper to which I allude is entitled, "Notes on the Cretaceous and Carboniferous rocks of Texas, by Jules Marcou,"—and was published in the VIIIth volume of the Proceedings of the Boston Society of Natural History. It is intended as a critical review of a paper by Dr. B. F. Shumard, containing a section of the Texas formations; and the personal remarks to which my attention has been called, are made incidentally in connection with this review.

The first of these remarks worthy of notice, (and it is of no very great importance,) occurs on page 94, where he mentions an error on one of the plates of the VIIth volume of the Pacific Railroad Reports, in copying a figure from a paper of his in the Bulletin of the Geological Society of France, citing it as an instance of the careless work of "Messrs. Hall, Blake and Meek."

Now the fact is, the figures referred to by Mr. Marcou, were not copied by me, as I have already explained to him by letter. The other figures on the same plate were drawn by me from specimens left by Mr. Blake at Albany, N. Y., where I was then living. Each of my drawings was on a separate piece of paper when they went out of my hands. The other figures from Mr. Marcou's paper, were copied and arranged on the same plate with them by some person at Washington City; and it was not until I saw a proof of the plate sent on to Albany, that I was aware these figures had been copied from Mr. Marcou's paper.

After the publication of the volume containing this plate, I received a letter from Mr. Marcou, who was then in Switzerland, complaining that I had received credit for drawing a plate in it which contained a few figures that had been copied from a paper of his. To this I replied, informing Mr. Marcou that the figures referred to by him, were not copied by me, and that I was not aware my name would be mentioned in connection with *any* of the drawings in this report, until I saw it in print.

As I received an answer from Mr. Marcou (dated Zurich, July 1st, 1858, which is now in my possession) acknowledging the reception of my letter, and expressing satisfaction with the explanation in regard to the plate, I am a little surprised that he should now connect my name with it; though it is possible the nature of our correspondence on the subject may have escaped his memory.

On page 96 of his Boston paper already alluded to, Mr. Marcou, in speaking of the so-called *Gryphæa Tucumcarii*, says, "On looking more

closely at the plates of the *Mexican Boundary* Report, I found on the last plate, No. XXI, fig. 3a, 3b, 3c, a specimen of *Gryphæa Tucumcarii* under the false name of *Gryphæa Pitcheri*. Mr. Conrad in his description of *Gryphæa Pitcheri*, p. 155, makes no reference whatever to that plate, nor to the figures 3a, 3b, 3c; and in the explanation of Prof. Hall's Report, p. 174, nothing is said of the locality or the stratigraphical position of this fossil. The plate was drawn by Mr. F. B. Meek, who has put it under the head of *Cretaceous*."

He even goes on to intimate that I had either myself, or in connection with another party, added the figures of this fossil to Mr. Conrad's report, without his knowledge or consent; and that too (as he thinks) under a wrong name, and in a wrong place. He also quotes a letter of Mr. Conrad's in such connection as to convey the impression that Mr. Conrad intended to intimate the same thing. Thus he says, "desirous to know the opinion of Mr. Conrad on this incomprehensible and doubtful proceeding, I wrote him, and give below his answer:"—

"Philadelphia, January 25th, 1861.

JULES MARCOU, Esq.

Dear Sir. . . . . When I drew up the report in Emory's Survey, I was shown by Prof. Hall a series of *Gryphæa*, some of which were undoubtedly your *G. Tucumcarii* as figured on plate XXI. Prof. Hall thought they graduated into *G. Pitcheri*, and I thought so too at that time. The name of your species ought not to have been placed as a synonym to plate VII, figure 3, for it is undoubtedly *G. Pitcheri*.

But the figures on plate XXI, represent a species and specimen, the locality of which is *unknown* to me, and were engraved *after* I had sent in my report and descriptions, so that I cannot now say that I do know whether *G. Pitcheri* is identical with your species or not.

The locality of the *G. Pitcheri* (page 155 Leon Springs, Texas; Plains of Kiamesha, Arkansas; New Braunfels, Texas; Fort Washita and Cross Timbers, Texas;) are correctly given from MSS. accompanying the specimens.

. . . . Sincerely yours, T. A. CONRAD."

Having had nothing whatever to do with the Mexican Boundary Report, excepting to draw the fossils figured in it, while all my drawings were on separate pieces of Bristol board, none of which were arranged on the plates by me;—and having had no agency whatever in deciding which figures should be arranged under the head of Cretaceous or otherwise,—I knew it was utterly impossible that Mr. Conrad could have even suspected, when he was writing his letter quoted above, the nature of the construction Mr. Marcou would place upon it. Consequently on seeing Mr. Marcou's paper I wrote Mr. Conrad on the subject, and received the following reply:—

"Trenton, June 21, [1861.]

My Dear Sir,—I am surprised that your name has been introduced in reference to the Palæontological report in Emory's Boundary Survey. You certainly had no connection with it other than drawing the plates, and never, that I know of, expressed an opinion of any species named in that report. When I said that *G. Tucumcarii* ought not to have been placed as a synonym to plate VII, fig. 3, I meant that *I* should not have done so, as I was not entirely satisfied of the identity of *G. Pitcheri* and *G. Tucumcarii*. I alone am responsible for the error, if there be one. I either made the reference myself, or authorized Prof. Hall to make it in my name. The figure on plate XXI, to the best of my recollection was engraved *after* I had sent in my report, but no one excepting myself is responsible for omitting any reference to it in my report.

Truly yours, T. A. CONRAD.

In a postscript to this letter Mr. Conrad also states, amongst other things, that he is now satisfied that the so-called *G. Tucumcarii* is the

typical form of Morton's Cretaceous *G. Pitcheri.* The following are his words: "by-the-by, Gabb showed me specimens which convince me that *Tucumcarii* is the original form of Morton's *G. Pitcheri.*"

So it will be seen that what Mr. Marcou calls "the mysterious appearance of this beautiful fossil" under the head of Cretaceous, in Mr. Conrad's report, is not, after all, so "incomprehensible and doubtful" a matter as he had supposed.

Mr. Conrad's latest opinion, after examining additional specimens that the so-called *G. Tucumcarii* is the typical form of Morton's *G. Pitcheri*, and that his own *G. Pitcheri* var. *navia*, (that is, the narrow form Dr. Roemer and Mr. Marcou referred to *G. Pitcheri*) is probably a distinct species, is in exact accordance with my own views expressed as long back as 1858. Mr. Gabb has also arrived at the same conclusion;* and when it is borne in mind that he and Mr. Conrad have at Philadelphia Morton's original specimen, and an extensive series of the *Gryphœas* of various ages from Texas and Arkansas, for comparison with Mr. Marcou's figures, drawn as he states by one of the best artists in Paris, the impartial reader will readily understand the value of their opinion on this point.

Mr. Marcou lays great stress upon the differences between the so-called *G. Tucumcarii*, and the narrow form formerly referred by him to *G. Pitcheri*, as if this has some bearing on the question at issue in regard to his Pyramid Mountain Jurassic. Whether we regard these forms, however, as distinct, or only as varieties of one species, has no bearing whatever on the main question at issue, since it is well known, as has been shown by Dr. B. F. Shumard, that they are both Cretaceous, and occur together in the same bed in Texas along with the so-called *Ostrea Marshii*, and numerous well marked Cretaceous fossils. Dr. S. has also shown that undoubted Cretaceous genera and species also occur in Texas, far beneath the horizon of the *Ostrea* and *Gryphœa*, Mr. Marcou supposed to belong to the Jurassic.

In regard to Mr. Marcou's efforts to explain away these facts, and to show that Dr. Shumard is in error as to the order of succession of the Texas formations, it is unnecessary for me to say a single word, as that geologist is abundantly able to defend his own views.

That Jurassic, and possibly Triassic rocks occur in New Mexico and other portions of the southwest, is very probable, but what Dr. Shumard, Dr. Newberry, Mr. Conrad, Mr. Gabb, myself and others maintain is, that the fossils relied upon by Mr. Marcou to prove the existence of the Jurassic at Pyramid mountain, are Cretaceous species; and consequently that he has included Cretaceous strata in his Jurassic. We have undoubted Jurassic fossils from the Black Hills in Nebraska, and from a few other points on the north Platte above Fort Laramie, as well as from some places in Utah, but they all differ widely from those referred by Mr. Marcou to that epoch, and hold a much lower stratigraphical position. No Jurassic fossils have yet been found in the country west of the Mississippi, south of Utah, so far as my knowledge extends, unless some remains of plants, and large Saurian bones discovered by Dr. Newberry in New Mexico, belong to that epoch; and these came from the red beds far below the horizon of Mr. Marcou's supposed Jurassic fossils.

I remain, yours truly, F. B. Meek.

Washington, D. C., July 27, 1861.

* Proceed. Acad. Nat. Sci. Phil. Feb. 1861.

2. *Note on Mr. Lesquereux's Table of Comparative Sections of Coal Measures.* (To the Editors of the Am. Jour. Sci. and Arts.)—Throughout the notice of the Kentucky Geological Survey, published in the July number of your Journal, my name is perhaps liable to be confounded with that of my brother Joseph Lesley, Jr., to whom is due whatever credit belongs to the Topographico-geological report of the margin of the eastern coal field of Kentucky.* As in this notice, quotations from my friend Mr. Lesquereux refer several times to my little Manual of Coal and its topography, published in 1856, I trust that you will permit me a word or two in relation to its exhibition of the Pennsylvania measures. The sections referred to in the Manual are most of them from ten to twenty years old, and were offered as illustrations of principles, not as guides to local geologists. They were meant to exhibit the general scheme of the Coal Measures; but neither settled the principal questions mooted among us, nor embodied late discoveries. One of these is of such importance that I wish to call special attention to it here, as I have done once already at the Montreal Meeting of the American Association.

It will be noticed in Mr. Lesquereux's "Table of Comparative Sections" accompanying your notice, and in the first two columns representing the Kentucky Coals, that a group of beds, four in number, seem to be represented in the other columns by only one bed, the Pennsylvania Pittsburg bed.

Five years ago I made a careful survey of the Coal series in Somerset County, Pennsylvania, especially of its upper and middle part. I found here a group of beds, one of which was the Pittsburg bed, exactly like the group No. 9, 10, 11 and 12 of Western Kentucky, with the Anvil Rock in character above it. The group consisted of three large beds, ten, twelve and sixteen feet thick respectively, with intervals of fifty and sixty feet between them; and, with these, just under the Anvil Rock, was a fourth and smaller bed. I have never had time to write out for publication my notes of this survey, nor indeed of any of my surveys; but no region that I have examined is more worthy of description, because of the flagrant representation which it makes of the persistency of our coal beds over a region at least seven hundred miles wide from east to west; affording a satisfactory demonstration, that the middle Kentucky area, or Cincinnati Axis as we used to call it, has been denuded of the same coal measures, deposited in the same order, and of about the same thickness as now exist at widely separated points on both sides of it; namely, at the summit of the Alleghany Mountain and at the mouth of the Tennessee river. I have always considered the theory of the dying out of the coal-beds and intervening rocks along the western outcrop of the S.E. Ohio and E. Kentucky coal area to be contrary to sound and wide geological views. I have always doubted or disbelieved the few local data supposed to favor that theory, drawn from the vicinity of Greenup County, Kentucky. My own observations at Hanging Rock were against it. My brother's very

* Peter Lesley, the author of a "Manual of Coal and its Topography," and one of the original corps of Assistants on the Pennsylvania Geological Survey in 1839—has since 1848 written his name J. P. Lesley—No careful reader of the notice alluded to on p. 118 need be perplexed in distinguishing between this gentleman and his accomplished brother Joseph Lesley, Jr., author of the Topographical Geological Report of Kentucky.—Eds.

complete survey of the outcrop across the whole State of Kentucky shows nothing of it. On the contrary, everywhere along the great escarpment line of the Ohio, Kentucky and Tennessee outcrop, denudation reigns supreme as limiting the coal area westward, without observable assistance from nonconformability. And certainly the exact agreement of the great group of upper beds and Top Rock of Somerset County, Pennsylvania, with the great upper group and Anvil Rock of Western Kentucky, must convince every geologist that one continuous coal area stretched from the head of the Potomac to the mouth of the Tennessee, when the Pittsburg bed and its group were made. I am happy to say that in this faith I agree with Mr. Lesquereux.*

There are some other points which I wish to touch on while occasion offers. My friend Mr. Lesquereux wishes to suppress the name False Coal Measures. I agree with him. I never liked the term. I used it because there was no other. In fact no other has been suggested, except in connection with some whole nomenclature, such as Mr. Rogers's Vespertine coal. But then it has a well-known meaning, and is understood by everybody. Geologists always talked of the Coal Measures. But a day came when a whole series of carboniferous rocks were discovered not belonging to the Coal Measures, and in fact thousands of feet below the acknowledged base of the Coal Measures. These rocks had deceived everybody, and would still deceive many. They were therefore rightly called at first False Coal Measures. Geologists will recall how completely they deceived the Nova Scotia geologists, even into inventing an extraordinary theory of lagoons and back deposites. They still exist, and they must have some name. They are still deceptive. What shall we call them?

But Mr. Lesquereux is not even in favor of acknowledging their existence as a separate series. He wishes to include them in the Coal Measures. He says the distinction is invalid, because it is based upon the fact that they are not generally found over the whole extent of the coal-fields of America. I think my friend is wrong. He underrates their extent. And then again the distinction, I take it, is not based upon horizontal considerations but upon vertical ones; based upon the immense interval between the two series as observed along their Alleghany mountain outcrops. He will recollect that at the head of the Juniata up the gorge of Tipton Creek, there are two open gangways in what are probably *two* coal-beds three feet thick, *six hundred feet below the great conglomerate* at the base of the Coal Measures. At Augusta Springs in Virginia, and from Blacksburg and Christiansburg southwestward through Wythe County, Virginia, there are False Coal Measures beneath at least two thousand feet of Red Shale and limestone, and how much more cannot be told because of the vicinity of those immense faults which bring up lower Silurian rocks against the coal. Now these False Coal Measures are surely an independent system; and as such they correspond with a similar independent system in Europe. I believe my friend Mr. Lesquereux has determined their independence also by at least one fossil plant which he has never seen above the Red Shale; that is in connection with the True Coal Measures. This system although to all appearance, full 1500 feet lower in the series than the Tipton coal, cannot be considered other than above it; inasmuch as the Red Shale is only about one hundred feet

* See his paper in this Journ., vol. xxviii, pp. 28, 29.

thick at Tipton, and just beneath the conglomerate. But we may easily imagine that the Tipton coals are the actual representatives of the Virginia system, by considering the thousand feet of Red Shale of the latter to be a lithological enlargement, southward, of the Tipton Red Shale, at the expense of the five hundred feet of Tipton green sandstones which do not appear above the Virginia coals. But there is still another series to be spoken of.

Between the False Coal Measures and the True Coal Measures by which I mean the carboniferous system above a supposed persistent conglomerate stratum, (now however known to be a variable group of sand rocks, shales and coals), there is what might and what would be called a third coal system, if the conglomerate proved to be a universal horizon rock. For underneath the conglomerate, and at the top of the Red Shale, formation XI, (Rogers's Umbral), lie alternations of carboniferous, argillaceous and siliceous shales, containing at least one real coal-bed. This appears everywhere beneath the conglomerate or the sand-rocks at the base of the Coal Measures.

The question for my friend Mr. Lesquereux to settle for us is this:—Whether the coal systems under the conglomerate in eastern Kentucky, as described in my brother's report and shown in his long section, is this sub-conglomerate system of Pennsylvania, and is perhaps also represented by the low Mercer and Trumbull County coals in the Lake Erie district, at Massillon, &c.;—or whether it is the Wythe and Montgomery County sub-red-shale False Coal Measures of Virginia;—or whether it is the reappearance in Kentucky of the Tipton Juniata coal-beds at a still lower horizon;—or whether it *represents all these* in a general thinning out of the Devonian and sub-carboniferous, westward, from the massive outcrops of the Alleghany.

I am sorry that I cannot agree with my friend Mr. Lesquereux when in continuing his argument against the name False Coal Measures he says that "the same [objection that they do not extend over all the coal region] can be said of the coal strata between the Mahoning and the Anvil Rock sandstone, and particularly of the upper Coal Measures above the Anvil Rock." I have already adduced the Somerset County coals to demonstrate the contrary respecting the Pittsburg group of beds (between the Mahoning S.S. and the Anvil Rock); and it seems to me that the contrary is equally clear respecting the group of beds above the Anvil rock; for these beds exist in Green and Washington counties, Pennsylvania, along the Monongahela river, six hundred miles away from the West Kentucky coalfield; and I believe them to exist in the Pottsville Anthracite basin. I sympathize heartily with my friend Mr. Lesquereux in his dislike to the erroneous depths which have been assigned to the Anthracite Measures. But I fear I cannot go all lengths with him in this dislike, when, if I understand his language, he makes the Pottsville basin so shallow as to contain only four nominal coal-beds, or rather the equivalents of the beds from the Kentucky coal No. 4 downwards. This must be a very great mistake, which I think Mr. Shæffer or any other geologist in that region can demonstrate without trouble. If Mr. Rogers has repeated and endorsed the old blunder of separating the Mammoth and Jugular Veins, thereby duplicating the lower part of the meas-

ures in the Pottsville basin, and has also given by far too much latitude in his measurements to the faulty condition of the middle of the basin, the incredible thickness of the measures which he thereby obtains should not drive us to the opposite extreme. Now when Mr. Lesquereux says: "from palæontological evidence I am satisfied that the highest coal of the Pottsville and Tamaqua basin is the equivalent of our No. 4, and that the measures do not ascend higher in that part of Pennsylvania," he merely destroys the value of palæontological evidence to the eyes of those of us who have been so long and so lovingly and reverently watching his footsteps on our mountains, bringing us glad tidings of a new science. He certainly forgets that at Shamoken he can count twelve great beds of coal from below upwards, and then can lay his hand upon a conglomerate, which if lithological laws are of any value, must some day prove to be very near his Anvil Rock. He must not forget that even in the shallow synclinals of the Lehigh mountains he has a greater depth of coal than he here assigns to the wide and deep Pottsville and Tamaqua basin. He must not forget that the sharp mountain exhibited to him in every one of its gaps, a row of vertical coal-beds, long enough to more than include the sub-Anvil rock group. It seems as well demonstrated as the absence of fossils will permit it to be, that we have in the middle folds of this great anthracite basin, in either the group of the Salem vein, or of the Peach Mountain, the representatives of the super-anvil-rock coals of western Kentucky, and of the Green and Washington County coals of southwestern Pennsylvania.

In a letter just received (Aug. 20th) Mr. Lesquereux informs me that when last in Pottsville, Mr. Schaffer and other gentlemen, coal proprietors, assured him that the Salem, Gate, and Tunnel veins, which in the face of every opposition he had always identified, were now found to be in fact the same, or belonging to the same group. In that case we must permit him certainly to place them in as low a botanical position as he has. This will not invalidate the series made out from the northern and undisturbed flank of the basin, along the Mine Hill. It will leave, in fact, the basin as deep as any in the United States.

My friend thinks and says, with the enthusiastic confidence of a master in palæontology, which he is,—in fact our great master in the botany of the coal,—that we structuralists can do nothing to help the great questions of East and West to a solution; that "it is only by palæontology that the equivalency of the coal strata has been and can be established in distant parts of the same basin, and especially in separate coal basins." If this be said exclusively of the drift-covered basins of the far west, it is reasonably true; but when stated broadly and made therefore applicable to the whole coal area it is surely unacceptable. Palæontological identification is certainly the crowning glory of our science, the *abendröthe* of our working day, and the reward we receive for patient toil in one department by finding at the far end a door thrown open for us into a new and finer one. But let every tub stand upon its own bottom. Palæontology and Lithology must learn to live and let live. My friend's botanical conclusions are some of them yet to be modified by structural examinations; as in fact they were reached through structural observations. The patient tracing of outcrops from ravine to ravine, and from county to county; the patient

construction and comparison of cross sections on hillside after hillside, until not an exposure has escaped; the patient triangulation and levelling of area after area until a perfect map of the surface has been obtained; and the patient underground working up of gangways and breasts, tunnels and shafts, which reveal blunders no surface exhibitions could have corrected:—What is the best palæontological work in comparison with all this—*for results?* What is even the patience, the skill and the genius of Lesquereux, for a life-time, to a combination of apparatus and opportunity so overwhelming and complete? The "comparative sections" in the table were all constructed with the very slightest references possible to palæontological evidence. The Freeport-Curlew limestone and the Ferriferous limestone are great *lithological* horizons, entirely established by lithological and structural geology.

The opinion expressed by my friend Mr. Lesquereux "that coal No. 1, with its members B and C, and perhaps No. 2 subdivides, forming as many as eight different strata," is an evidence how little structural difficulties appall palæontologists. To my mind, filled with the experiences of twenty-five years in coal, such a thing is simply incredible. So far as my observation has covered the ground, no coal-bed is known to separate permanently into two or more unless it is essentially a double or triple bed; that is, unless it evidently consists, at the points where it seems to be but one bed, of two or more, by virtue of persistent interlaminations of fire clay, or some equivalent rock. I have never seen an exception to this rule. The great bed of the lower anthracite coal is a permanently triple bed, and therefore its members are sometimes separated by twenty or thirty feet of slate. The great Mauch Chunk Summit Mine Coal is, even to the uninstructed eye, a group of beds, spreading somewhat further asunder elsewhere. But that any one of the small western bituminous coals should first so separate in parts as to obtain regularly lettered or numbered "members" and then these again split up until the one bed should be represented by a tall cross section containing eight separate beds of coal—I cannot easily believe it.

In conclusion I wish to suggest to geologists who take a special interest in the carboniferous formation, that the term Mahoning sandstone has become as unsafe a name for a horizon line, as is the term Conglomerate. My surveys for the Pennsylvania Railroad Company some years ago, in Indiana, Fayette and Westmoreland Counties, made me aware of several important rocks in the Barren Measures by which that whole group, (and I must still consider it an *interval* separating most decidedly the upper and lower divisions of the true Coal Measures), can be discussed intelligently. One of these rocks is a conglomerate of weight and character, appearing over a considerable country, and forming as I believe, as general a horizon as any eminent sand-rock can. It underlies the Pittsburg coal-bed one or two hundred feet; overlies the red bands of the Barren Measures; forms the coping rock along the centre of the Ligonier valley north of the Conemaugh, and crowns with cliffs the ravines of Castleman's river, in southern Somerset, above the Turkey-foot. For a long time I confounded it with the Mahoning sandstone, the true place of which is from one to three hundred feet further down. But I afterwards learned to distinguish and value it apart.

J. P. Lesley.

Philadelphia, Aug. 1, 1861.

3. *On the origin of some Magnesian and Aluminous Rocks;* by T. STERRY HUNT, F.R.S., of the Geological Survey of Canada. (From the Canadian Naturalist for June, 1860.)—In common with other observers, I have long since called attention to the fact that silicates of lime, magnesia and oxyd of iron are deposited during the evaporation of many natural waters, such as the mineral springs of Varennes and Fitzroy, and the waters of the Ottawa river. I have also suggested that the silicates thus produced may have contributed in a considerable degree to the formation of rocks. (This Journal, March, 1860, p. 284). A hydrous silicate of magnesia which approaches in composition to $MgO\ SiO^3$, combined with from ten to twenty per cent of water, and mechanically mixed with small portions of oxyd of iron, alumina and carbonates of lime and magnesia, forms extensive beds with limestones and clays in tertiary strata, in France, Spain, Morocco, Greece and Turkey. It is the sepiolite of Glocker, the meerschaum of some authors, the magnesite of others. The quincite of Berthier, which occurs in red particles disseminated in limestone, is a similar compound, containing some oxyd of iron. The sepiolite from the basin of Paris occurs beneath the gypsiferous group, and in the lacustrine series known as the St. Ouen limestone, where it forms very fissile shaly layers, enclosing nodules of opal (menilite). The structure of this sepiolite, which I have examined and described as above, and that from Morocco, which is used by the Moors in their baths as a substitute for soap, and has been described by Damour, is peculiar. The mineral is made up of thin soft scales, and when moistened with water, swells up into a pasty mass resembling a finely divided talc. Although agreeing closely with this mineral in the proportions of silica and magnesia, sepiolite contains more water, and both before and after ignition is soluble in acids, which talc is not. We cannot however doubt that talc and steatite have been formed from sepiolite, which has undergone a chemical change and become insoluble. It is possible that serpentine may be derived from another silicate richer in magnesia than sepiolite. The frequent association of carbonates of lime and magnesia with talc, and of carbonate of magnesia, talc and serpentine, as in the ophiolite of Roxbury (This Jour., [2], xxv, 224,) would seem opposed to the notion that serpentine may have been formed from the alteration of a mixture of sepiolite and carbonate of magnesia. In chlorite, which often forms rock masses almost without admixture, we have an alumino-magnesian silicate which cannot have been derived from sepiolite, inasmuch as this contains for the amount of magnesia present, twice as much silica as chlorite. The oxygen ratios of the silica and magnesia in sepiolite are as 3 : 1, and those of silica, alumina and magnesia (including the variable amount of ferrous oxyd which in part replaces the latter) in chlorite are as 6 : 3 : 5, while in the purest clays the ratio of silica and alumina equals 1 : 1, and in most argillaceous sediments the proportion of silica is still greater. It is evident, therefore, that chlorite could not be formed from a mixture of sepiolite with clay, or even with pure alumina, without the elimination of a large amount of silica, and we are led to regard it as having been generated by the reaction of a silicate of alumina or clay with magnesia, which was probably present in the unaltered sediment in the form of carbonate. Unless indeed the process, which according to Scheerer, has in recent times caused the deposition from waters, of

neolite, a hydrous alumino-magnesian silicate approaching to chlorite in composition, be the type of a reaction which formerly generated beds of chlorite, in the same way as those of sepiolite or talc.

A silicate of lime allied to sepiolite, has not so far as I am aware, yet been noticed among unaltered sediments, and among crystalline strata calcareous are more rare than magnesian silicates, although double silicates of lime and magnesia (pyroxene and hornblende,) often form beds, and wollastonite, either alone or mingled with carbonate of lime, sometimes constitutes rock masses. The double silicates of alumina and lime are however abundant; the lime-feldspars, scapolite, epidote (saussurite), and white garnet, all form beds in crystalline rocks. Reactions in water at the earth's surface, and at no very elevated temperature, may have given rise to double silicates of lime and alumina corresponding to neolite, and allied in composition to the zeolites, and these by subsequent metamorphism have been changed into anhydrous silicates. The production of harmotome, chabazite and apophyllite by the waters of a spring at Plombières, at temperatures not above 160° F. as observed by Daubrée, lends probability to such a view.

But while we admit the possible direct formation of double silicates in water at ordinary temperatures, there is not wanting evidence that the reaction which we long since pointed out, (Proc. Royal Society of London, May 7, 1857, this Jour., [2], xxv, 287), between silicious and argillaceous matters and earthy carbonates in presence of alkaline solutions, intervenes in the metamorphism of sedimentary rocks and in the production of many silicious minerals. The blue Silurian limestones of the island of Montreal, when treated by acids leave an insoluble residue, which contains about ten per cent of soluble silica, mixed with an argillaceous matter whose analysis gave silica 73·0, alumina 18·3, potash 5·5, and only traces of lime and magnesia. In the vicinity of an intrusive dolerite, however, the limestone is changed in color, and leaves by the action of acids a greenish matter, which consists of silica 40·2, alumina 9·3, peroxyd of iron 5·2, lime 36·6, magnesia 3·7. The free silica and that of the intermingled aluminous silicate, has thus been saturated with protoxyd bases, still, however, retaining the alumina in combination. A similar reaction with more aluminous matters, would give rise to epidote, garnet, magnesian mica, scapolite or feldspars like labradorite and anorthite, and it is not impossible that in such reactions a portion of alumina may sometimes be set free, and give rise to corundum, spinel, diaspore or völknerite.

In the ordinary modes of decomposition of minerals containing alumina, this base separates in the form of silicate, and the conditions required for its elimination in a free state are but imperfectly understood. We have elsewhere pointed out the decomposition by alkaline and earthy carbonates, of solutions of sulphate of alumina or native alum, as one source of free alumina, and insisted upon the existence of pigotite, a native compound of alumina with an organic acid, as an evidence that this base is sometimes like oxyd of iron, and oxyd of manganese, (this Jour., [2], xiii, 12), taken into solution by water aided by organic matters. A hydrate of alumina, gibbsite, is found associated with limonite, and the aluminous minerals from the south of France described by Berthier and Deville, show that free alumina is much more common in

nature than was formerly supposed. Berthier long since gave the name of bauxite to an earthy pisolitic ore which occurs either massive or imbedded in limestones of tertiary age, at Baux, and many other localities in the departments of Gard and Var, and also in Calabria, and the Grecian Archipelago, forming in some places an abundant rock.* This substance is a variable mixture of a hydrate of alumina, apparently approaching diaspore in composition, with hydrous peroxyd of iron, sometimes constituting a workable iron ore, and at other times a veritable ore of alumina. It contains besides small portions of silica, titanic, vanadic and phosphoric acids, and occasionally encloses grains of corundum. A compact dark red variety gave Deville, alumina 57·6, peroxyd of iron 25·3, and water 10·8, besides 3·1 of titanic acid, and 2·8 of silica. In other specimens the proportions of alumina and iron oxyd are nearly equal, or the latter predominates, as in one example where the proportions were 48·8 of iron oxyd, and 32·2 of alumina; and another, 60 of iron, and 18 of alumina and titanium. In these analyses the carbonate of lime, generally present, was first removed by a dilute acid; the prolonged action of stronger acids completely dissolves the hydrated oxyds. By an intense heat this substance is converted into crystalline corundum, resembling emery in its physical character, but the presence of grains of corundum in the hydrated mineral seems to show that this transformation may take place at ordinary temperatures. The emery of Greece and Asia Minor, which is associated with variable proportions of oxyd of iron, is according to Dr. J. Lawrence Smith always more or less hydrated.

The argillaceous matter enclosing some varieties of this bauxite or impure diaspore, is white, without plasticity, and very rich in alumina; one specimen freed from the red ferruginous portions, gave alumina 58·1, silica 21·7, peroxyd of iron 3·0, titanium 3·2, water 14·0. This substance approaches in its composition to collyrite, and to the dillnite which is the gangue of the diaspore of Schemnitz. These materials however contain from 20 to 40 per cent of water. Scarbroite, schrötterite, and allophane are similar matters; the latter, unlike a clay in its structure, appears to have been deposited from solution. The subsulphate of alumina, known as websterite or aluminite, is often met with in layers and concretionary masses in Tertiary clays,† and is sometimes mingled with a silicate having the composition of allophane. This frequent occurrence of alumina still retaining a portion of sulphuric acid, confirms the view which we have elsewhere expressed, that solutions of native alums have by their decomposition furnished the alumina for many of the minerals in question, while the conditions under which this base is taken into solution by organic matters, still require investigation. The careful examination of unaltered sedimentary deposits, is calculated to throw great additional light upon the origin of the crystalline rocks.

4. *Note to the Paper of Messrs. Meek and Worthen on the Age of the Goniatite Limestone*, p. 167 of this No.—The name "Kinderhook Group" is now proposed by these authors to include the beds lying between the Black Slate and the Burlington Limestone, which have heretofore been considered the equivalents of the Chemung Group of New York. This designation will be observed in the Illinois Report.—(*Letter to Editors.*)

* Deville, *Ann. de Chimie et Physique*, (3) lxi, 309.

† In this connection we may notice apatelite, a basic persulphate of iron, which occurs in conditions similar to aluminite.

III. BOTANY.

1. *Martius, Flora Brasiliensis.*—Since our last notice, two more issues of this great work have come to hand: viz., fascicles 25 and 26, containing the *Santalaceæ* (two species of *Thesium*) and *Myristicaceæ* (26 species of *Myristica*) by Alph. DeCandolle, and the more numerous *Apocynaceæ*, which are thoroughly elaborated, with decided talent, by DeCandolle's curator, J. Müller, from whom excellent work may be expected, judging from this specimen. This was published in July, 1860. It is now followed by fascicles 27 and 28, published in February last. This contains the *Antidesmeæ* by Tulasne, consisting merely of three species of *Hieronymia* (the *Stilaginella* of Tulasne), the representatives of *Antidesma* in the new world, the group pretty clearly belonging to *Euphorbiaceæ;* also the *Begoniaceæ* by DeCandolle (83 species of *Begonia*), followed by a few pages of interesting disquisition by Von Martius himself. Finally we have the *Celastrineæ*, *Ilicineæ*, and *Rhamneæ*, by S. Reissek of Vienna, copiously illustrated, and with the leaves of many species self-printed by the Vienna process. The elaboration of these orders appears to be very satisfactory. We congratulate Von Martius upon the character and steady progress of this formidable work. A. G.

2. *Floræ Columbiæ terrarumque adjacentium Specimina Selecta*, edidit H. Karsten, Tom. I, fasciculus primus. Berol: F. Dümler, 1858. pp. 42, tab. 1–20, col. imp., fol.—This promising commencement of a magnificent work has just reached us, with an advertisement by the successors of the original publisher, dated Feb. 1859, announcing that the publication, if the subscription warrants, will extend to ten such fasciculi, issued at intervals of six months; the price of each, with colored plates, twenty Prussian thalers; the uncolored copies, fifteen thalers. We trust the needful patronage will not be wanting. The plates are very fine, and with satisfactory analyses; and are accompanied each by two pages of letter-press; the characters in Latin; the detailed descriptions and remarks in the German language. The subjects are well chosen; the first representing two new Palms, species of Klopstockia from the Quindiu Andes; the second a Tree-Fern, *Cyathea ebenina* from near Caracas; eight others representing species of *Cinchona* or near relatives of this most important genus, and three others illustrate Rubiaceous genera, *Poggendorffia rosea* seems to be a good *Tacsonia*. The pretty Cobæaceous *Rosenbergia penduliflora*, t. 14, with long ligulate lobes to the corolla and extremely long filaments, is in Fendler's Venezuelian collection, No. 468. A. G.

3. *Journal of the Proceedings of the Linnæan Society*, (Botany), No. 20, (1861) finishes Hooker and Thompson's precursory account of the Indian *Cruciferæ*, and contains five other papers; four of them very short ones, and of only local interest. Dr. Welwitsch's letter on the botany of Benguela, Western Africa, gives an account of a very queer dwarf tree, found near Cape Negro, the stem of which, "with a diameter often of four feet, never rose higher above the surface than one foot, and which through its entire duration, that not unfrequently might exceed a century, always retained the two woody leaves which it threw up at the time of germination, and besides these it never puts forth any other.

The entire plant looks like a round table, a foot high, projecting over the tolerably hard, sandy soil, the two opposite leaves, (often a fathom long, by 2 to 2½ feet broad) extend on the soil to its margin, each of them split up into numerous ribbon-like segments. The flowers are hermaphrodite and collected in a sort of aments or strobiles." There has also appeared a *Second Supplement* to the botanical portion of vol. v, (1861) which contains four very interesting papers, viz:

(1.) The Natural Order *Aurantiaceæ*, with a Synopsis of the Indian Species, by Daniel Oliver, the newly appointed Professor of Botany in University College, London, who here takes rank as a first-rate monographer.

(2.) Notes on *Menispermaceæ*, by George Bentham, who now, as botanists will be pleased to learn, occupies the presidential chair of Brown and Smith in the Linnæan Society. That the so-called opposition of the parts of the flower in *Menispermaceæ* and *Berberidaceæ* is only in seeming, must we suppose be every where understood and admitted. As to the opposition of stamens to petals in *Rhamneæ* and the like, if shut up to the alternatives mentioned by Mr. Bentham, we should still prefer "the idea of a common origin" to that of the stamen being developed in the axil of the petal, which would introduce a needless and most improbable complexity into the morphology of the flower. But a third alternative, which was long overlooked, appears to offer a more natural explanation, conformable with the laws of the arrangement of leaves, viz., that the stamens are here normally superposed just as the parts of the successive cycles of alternate leaves are,—the ordinary alternation of floral parts being really the thing to be accounted for.

(3.) Notes on *Tiliaceæ*, and 4, Notes on *Bixaceæ* and *Samydaceæ*, by the same author, are masterly papers. At the first view we were far from satisfied throughout with the proposed re-arrangements, but the more we consider the matter the better we like it, even in the association of *Cochlospermum* and *Amoreuxia* with *Bixa*. We should still prefer the union of the *Sanydaceæ* with the *Bixaceæ*, and make less than Mr. Bentham does of the difference between hypogynous and perigynous insertion,—a difference often obscure or shading away, and as often coëxisting in the same natural group, or among closely related ones, as parietal and axile placentation do. No one knows this better than the sagacious authors of the forthcoming new Genera Plantarum; but their adhesion to the old tradition is compelled by their determination "to maintain the Candollian series in its general features,"—which is certainly still desirable; but if rigidly followed out in this respect will certainly do much violence to natural affinities, perhaps without securing adequate practical advantages. But it is far easier to interpose objections than to collocate the natural orders upon any consistent system. A. G.

4. *Calluna vulgaris and Aira caryophyllea in the United States.*—That "America has no Heaths" is a botanical aphorism. It is understood, however, that an English surveyor nearly thirty years ago found *Calluna vulgaris* in the interior of Newfoundland. Also that De la Pylaie, still earlier, enumerates it as an inhabitant of that island. But this summer, Mr. Jackson Dawson, a young gardener, has brought us specimens and living plants (both flowering stocks and young seedlings)

from Tewksbury, Massachusetts, where the plant occurs rather abundantly over about half an acre of rather boggy ground, along with *Andromeda calyculata*, *Azalea viscosa*, *Kalmia angustifolia*, *Gratiola aurea*, &c., apparently as much at home as any of these. The station is about half a mile from the State Almshouse. Certainly this is as unlikely a plant, and as unlikely a place for it to have been introduced by man either designedly or accidentally, as can well be imagined. From the age of plants it must have been there for at least a dozen years; indeed, it had been noticed and recognized, two years ago, by a Scotch farmer of the vicinity, well pleased to place his foot once more upon his native heather. So that even in New England he may say, if he will—as a friend of ours botanically renders the lines—that

> "*Calluna vulgaris* this night shall be my bed,
> And *Pteris aquilina* the curtain round my head."

It may have been introduced, unlikely as it seems, or we may have to rank this Heath with *Scolopendrium officinarum*, *Subularia aquatica*, and *Marsilea quadrifolia* as species of the Old World so sparingly represented in the New, that they are known only at single stations,—perhaps late-lingerers rather than new-comers.

Aira caryophyllea has just been detected by Wm. M. Canby, Esq., in New Castle county, Delaware, growing very abundantly, "in a dry piece of ground, rather open, but dotted over with pine trees (*Pinus inops*), and completely surrounded by a forest. It certainly has not been ploughed for ten years, probably not for a much longer time. In company with it was *Polygala Nuttallii*, *Sorghum nutans*, &c., but no clover, timothy, or any of the grasses usually cultivated. Still we suppose this species to have been introduced. A. G.

IV. ASTRONOMY AND METEOROLOGY.

1. *The recently discovered Asteroids.*—In the last No. of the Journal we gave a list of the asteroids discovered since 1858, together with the elements of several of them. We now give the elements of the remainder, and also corrected elements of Titania:

Berlin mean time.	(61) Titania, 1861, Jan. 0.2512.	(67) Asia, 1861, April 21.0.	(68) Leto, 1861, June 1.0.
L	38° 8′ 17″·29	241° 22′ 41″·4	240° 24′ 9″·1
ϖ	98 28 35 ·33	294 23 27 ·4	346 15 37 ·5
Ω	191 57 53 ·02	202 0 6 ·5	44 49 43 ·5
i	3 34 21 ·46	5 48 36 ·9	7 58 19 ·8
e	0·1847205	0·144085	0·185692
μ	958″·43568	958″·418	767″·637
a	2·393109	2·39314	2·77481

Berlin mean time.	(69) Hesperia, 1861, May 13.4864.	(70) Panopea, 1861, June 0.0.
L	160° 52′ 5″·0	253° 11′ 36″·7
ϖ	117 14 2 ·3	299 3 0 ·2
Ω	186 52 56 ·0	48 21 0 ·3
i	8 28 55 ·7	11 14 37 ·1
e	0.1779006	0.223545
μ	661″·260	813″·222
a	3·064905	2·670127

In the above tables,

L represents the mean longitude of the planet at Epoch.

π " the longitude of the perihelion.

Ω " the longitude of the ascending node.

i " the inclination of the orbit to the ecliptic.

e " the eccentricity of the orbit.

μ " the mean daily motion.

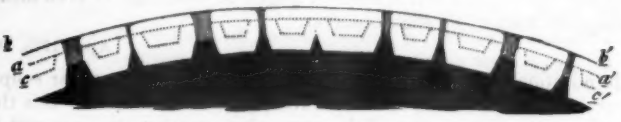
a " the semi-major axis of the orbit.

2. *Note Explanatory of Baily's Beads;* by EDMUND BLUNT.—We have received from Mr. Blunt the following satisfactory illustration of the cause of the formation of beads and ruptures of the ring, etc., in an annular eclipse of the sun. Although privately circulated, this explanation of Baily's Beads has not before been published.—*Eds.*

Brooklyn, N. Y., May 18th, 1854.

Mr. F. Baily concludes his paper, on the Annular Eclipse, May 15, 1836, thus, "That mountains may occasionally obtrude themselves to our view, there can be no question; but it requires further explanation why almost all of the remarkable distortions take place when observed near the edge of the Sun's disc, and as the Moon advances thereon, these prodigious elevations, in a great measure, disappear." The following is an explanation: (Fig. 1.)

1.



Let the line $a\,a'$ represent the true edge of the Sun's disc, and the exterior line $b\,b'$ the apparent edge, (the difference between the two being caused by the tremulous motion in our own atmosphere,) the zig-zag line $c\,c'$ the true edge of the Moon, and the dark the apparent edge, when seen on the Sun—the difference between the two being caused by the same.

Now, as the true edge of the Sun's disc touches any of the mountains on the Moon's edge, if the part in contact is *more* than twice this difference, dark lines will instantly shoot out and connect the apparent edges

2.

of the Sun and Moon, and they become wider as the Moon's edge approaches the exterior edge of the Sun's disc, beads being formed between, or narrower, as the Moon's edge recedes internally, until they break. Fig. 2 shows Baily's Beads at the instant of formation.

Fig. 3 shows the Sun's lower cusp as seen through an inverting telescope at New York, May 26, 1854.

3.

The following reasons, for the truth of this theory, I beg you to look at:

1st. The observed diameter of the Sun is always greater than the computed. See observations at various observatories.

2d. The diameter of objects projected on the Sun's disc are less than the computed diameters.

3d. Results, obtained from observations at the beginning and end of an eclipse are good; because the contact or end is not seen, except when the Moon's edge touches the true edge of the Sun's disc.

4th. Results obtained by the measurement of the distance of the solar cusps, &c., are not good, and, I think, it is entirely owing to their not correcting the angle, for increase caused by tremulous motion in our own atmosphere, and which amount might be estimated, or even measured during the eclipse, by each observer.

April 13th, 1860.

In addition to the above, I would remark that the annular eclipse, as observed September, 1838, in Brooklyn, showed the truth of this theory. The slow tremulous motion of the atmosphere causing the lines and beads to assume wavy shapes, the Sun's altitude, at the formation of the ring, being low. When the tremulous motion of the atmosphere is so rapid that the wavy lines do not appear, the shooting out of the black lines is almost instantaneous.

The truth of this theory, I tested in the following manner:—At the distance of about eleven miles, was placed a black-board, six feet by eight inches, with a hole in it, two-tenths of an inch in diameter, for the Sun's rays to be reflected through, towards me, with a heliotrope. Towards noon, when the atmosphere was tremulous, a large bright light only was seen, and, as the atmosphere became more steady, the size decreased, until the board began to show as a large faint shadow, which increased in intensity as it decreased in size, and at last showed the board well defined, with a bright point in the centre. The A. M. observations showed the same, but in reverse order.

The eclipse of July 17th (1860) will give an opportunity for observing these distortions, but it will be useless to look for them should there be any moisture, or even a slight mist in the atmosphere, extending over the earth's surface near the observer, as the Sun's diameter at such times, is not increased.

3. *Meteoric Observations, April* 20, 1861.—The following observations at New Haven show no recurrence of the meteoric shower of April 20, 1803, but they are too limited to be satisfactory. The shower may perhaps continue only a few hours, and thus be easily lost even if observers were watching throughout the globe.

Saturday, April 20, 1861. Observers: Mr. Edward R. Sill and myself. Sky clear; moon down. Different meteors observed:

$2^h\ 45^m$ to 3^h A. M.	in N. 4	S. 6
3^h " 4^h "	" 13	" 29

Of these about ten equalled in brilliancy stars of the first magnitude, but none were very remarkable. The majority appeared to come from near the zenith, about the constellation *Lyra*, but they did not show any decided radiant.

On the Monday morning previous, watching alone from $3^h\ 15^m$ to $4^h\ 15^m$, I saw only three or four shooting stars. E. C. Herrick.

4. *Meteoric Observations, August* 10, 1861.—(1.) *New Haven, Conn.*—At this place, the sky was clouded during the nights of Thursday and Friday, Aug. 8 and 9, 1861. The night of Saturday, the 10th, was clear and favorable. From the top of the tower of the Alumni Building, the following observations were made that night by a corps of four observers, viz. Messrs. Charles Tomlinson, Henry P. Johnston, Willabe Haskell, and myself. We were so stationed as to see as many as possible of the meteors, and yet some were undoubtedly lost. None were reckoned twice.

	N.W.	N.E.	S.E.	S.W.	
Aug. 10–11, 10^h to 11^h P. M.	22	26	16	12	= 76
11^h to midnight,	23	33	21	17	94
0^h to 1^h A. M.	23	38	29	29	119
Total seen in 3 hours,					289

Of these shooting stars the large majority moved in paths which traced back would intersect near the sword-handle of Perseus. Many equalled or surpassed in brightness stars of the first magnitude, and one at $11^h\ 30^m$, near the zenith, was much more splendid than Venus and left a train of sparks which remained luminous for twenty seconds after the meteor disappeared. This grand meteor was also seen at Providence, R. I., and was fortunately so well observed by Mr. B. V. Marsh at Burlington, N. J., that it will probably be practicable to ascertain its elevation, length of path and velocity.

Prof. A. C. Twining, of this city, observed here independently Aug. 10th and Aug. 11th, and will probably publish his results.

Watching alone from $8^h\ 30^m$ to 10^h P. M. of the 10th, I saw about 20 shooting stars, several of which were brilliant and accompanied by trains. There was, as heretofore noticed, a gradual increase of numbers from evening to morning.

(2.) *Burlington, N. J.*—Observers: Messrs. Benj. V. Marsh and Samuel J. Gummere. The observers, one looking S.W. and the other N.E., were stationed on the top of a house, but the view was somewhat impaired by obstacles. The sky was nearly clear until after midnight, when clouds began to appear in the South and Southeast. After 2^h they caused seri-

ous embarrassment, and by $2^h\ 45^m$ the sky was so nearly overcast that the observers retired. Different meteors observed:

1861.			N.E.	S.W.	
Aug. 10–11,	$10^h\ 25^m$ to	11^h P. M.	20	20	= 40
	11 "	12 "	29	29	58
	12 P. M. "	1 A. M.	47	38	85
	1 A. M. "	2 "	35	29	64
	2 " "	2 45 "	28	14	42
					289

Mr. Marsh, who was looking towards the N.E., writes: "At first I could not fix the radiant definitely, most of the tracks pointing to Perseus, but a number to Cassiopeia. Towards midnight it seemed to be very clearly within the triangle formed by η, γ, and τ Persei. I could not detect any change of position afterwards. We thought that seven-eighths of all we saw during the night were conformable. * * * Much the finest meteor seen was at $11^h\ 23^m$,—a most splendid specimen; its track remained visible twenty seconds or more."

Mr. R. M. Gummere, also at Burlington, N. J., watching independently, observed shooting stars as follows, viz:

Aug. 10,	$10^h\ 25^m$ to	11^h P. M.	34
	11 "	12 "	29
	12 "	1 A. M.	37

The night of Aug. 9–10 was stormy at Burlington.

(3.) *Natick, Mass.*—Observers: Messrs. F. W. Russell and E. L. Pray. The night of August 9th was cloudy and rainy. The night of the 10th was clear until about 2 A. M. of the 11th, when about a third of the sky was obscured by clouds. Between 8^h P. M. and $3^h\ 15^m$ A. M., the two observers saw 397 different shooting stars, as follows:

Aug. 10–11.			N.E.	S.E.	N.W.	S.W.	
8^h P. M. to	9^h P. M.		16	5	5	3	= 29
9 " "	10 "		10	2	2	8	22
10 " "	11 "		32	6	5	17	60
11 " "	12 "		28	13	3	30	74
12 P. M. "	1 A. M.		27	12	7	13	59
1 A. M. "	2 "		26	14	27	8	75
2 " "	3 "		14	27	12	8	61
3 " "	3 15 "		4	4	1	8	17
							397

During this period only about one-half the sky was under observation; and from 2 to 3 A. M. not more than half the sky was clear. Only four very large ones were noticed, twenty about the size of Jupiter, one leaving a luminous train of smoke for eight seconds, and the remainder quite small. The radiant, to which two-thirds of the meteors conformed, was a spot about three degrees in diameter, around *beta Persei*.

The foregoing observations show that the meteoric sprinkle of August occurred this year (1861), with the usual characteristics. The number may have been somewhat diminished, but was still very much above the average of ordinary nights. This average is greater than is usually supposed, and the actual number varies greatly on different nights. On

the night of July 3, 1861, four observers here saw in the hour ending at $10^h\ 5^m$ P. M. *thirty-six* shooting stars, or about half as many as are seen during the corresponding hour on the 10th of August.

It is to be hoped that observers, situated from 50 to 100 miles apart, will hereafter act in concert on the nights of meteoric abundance, so that in case of meteors remarkable for magnitude or other peculiarity, data may be obtained for determining their astronomical elements. E. C. H.

5. *Remarkable Rain-Fall in Ohio.*—(Extract of a letter from S. B. McMillan, dated East Fairfield, O., Aug. 15, 1861, to the Editors). "The same day (Aug. 12th) witnessed the greatest fall of rain ever recorded in this part of the State, in an equal time.

From noon to 1^h P. M.			there fell	0·32 in.	rain.
" $4^h\ 30^m$	" to $5^h\ 30$ P. M.		"	1·40 "	"
" $6^h\ 30$	" 9^h		"	2·85 "	"
" 9^h	" 11^h		"	3·44 "	"
Total,				8·01 "	" in 11 hours;

6·29 *inches of which fell in four and a half hours.*

This maximum rain-fall existed over at least 100 square miles. Few dams or bridges are left standing along the streams in this county (Columbiana)—many sheep, cows and horses were drowned, four or five dwelling houses were swept to destruction. The dead bodies of four persons of respectability, and the only persons known to have been drowned, were found the second day after the rain several miles below where the dwelling house in which they were swept away was crushed in pieces. In the village from which it was taken the water was seven feet higher than ever before known, reaching to the second story of nearly every house in the town. There was almost no wind here and only a moderate amount of lightning. A heavy wind is reported 30 miles east of here and also at Cleveland and Sandusky, where it did much damage."

6. *Rain following the discharge of Ordnance;* (note to the National Intelligencer, July 25, 1861.)—Messrs. Editors: In October, 1825, I took note of a very copious rain that immediately followed the discharge of ordnance during the celebration of the meeting of the waters of Lake Erie and the Hudson, upon the completion of the Erie canal; and in 1841 I published my continued observations on the subject, which, to my mind, fully established the fact that the discharge of heavy artillery at contiguous points produces such a concussion that the vapor collects and falls generally in unusual quantities the same day or the day following.

The early battles of the late war between the French, Sardinians, and Austrians were succeeded by such copious rains that even small rivers were not fordable; and during the great battle of Solferino a storm arose of such fierceness that for the time the conflict ceased. Within the last two weeks McClellan's columns on the upper Potomac fought four different battles on as many days, and there were extensive rains before the close of each day. July 21st the great battle of Bull Run, Virginia, was fought, and next day (22d) the rain was copious all day and far into the night. * * * * J. C. Lewis.

A more exact collection of such data as Mr. Lewis has here grouped may lead to important results equally in theory and practice.—Eds.

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Earthquake on the Island of Penang.* (In a letter to the Editors, dated U. S. Naval Observatory, Washington, 24th August, 1861.)—*Gentlemen*:—Mr. Geo. E. Tyler who has just returned from the East Indies, communicates to me the following:—

"*Saturday, Feb.* 16, 1861.—At 7½ P. M. there was a severe earthquake shock of sufficient force to throw down crockery from shelves, stop clocks, and to set articles to swinging. In walking, the ground seemed to undulate sufficiently to make many persons feel nausea. There appeared to be three distinct shocks, each of which seemed to pass from North to South and to continue 30 seconds.

"*March 2d.*—I have learned since the above, that the shock was felt at Malacca and Singapore at the same time at each place and with about the same degree of violence, though it was not recognized by many vessels in the harbors and on the passage here from Singapore.

"About five minutes before the shock, there was an unusual commotion in the sea and I spoke of it to others at the time. There was no perceptible air stirring, and the sky was clear. Thermometer 91° Fahr. No other earthquake has been known here during the last 17 years and then only a very slight one." Very respectfully yours,

J. M. Gilliss, *Supt.*

2. *Earthquake at Syracuse, New York.*—The Syracuse Standard of July 12th says: We learn from various sources that a very sensible shock of an earthquake was felt in this city and other parts of the county last evening, about nine o'clock. The weather yesterday very suddenly became quite cold and chilly, an extraordinary change from the intense heat of the previous days. The shock was about four seconds in duration, and was so severe as to cause dwelling-houses to rock, and in some cases furniture was removed and persons sitting in chairs were waved to and fro. Many persons supposed some of the fixtures of their dwellings had fallen upon the floors. A gentleman from the north part of the town of Salina informs us that the shock was sensibly felt in that section, and farmers ran out of doors supposing that their barns or outhouses had fallen.

3. *On the Natural Dissemination of Gold.*—Mr. Eckfeldt the Principal Assayer of the U. S. Mint at Philadelphia, has lately made several interesting examinations tending to show the very wide distribution of gold. Passing over the evidence respecting its presence in various galenas, in metallic lead, copper, silver, antimony, &c., we cite the following:—perhaps the most curious result of all.

Underneath the paved city of Philadelphia there lies a deposit of clay, whose area, by a probable estimate, would measure over three miles square, enabling us to figure out the convenient sum of ten square miles. The average depth is believed to be not less than fifteen feet. The inquiry was started whether gold was diffused in this earthy bed. From a central locality, which might afford a fair assay for the whole, the cellar of the new market-house in Market Street near Eleventh Street, we dug out some of the clay at a depth of fourteen feet, where it could not have been an artificial deposit. The weight of 130 grammes was dried and duly treated, and yielded one-eighth of a milligramme of gold; a very decided quantity on a fine assay balance.

It was afterwards ascertained that the clay in its natural moisture loses about fifteen per cent by drying. So that, as it lies in the ground, the clay contains one part gold in 1,224,000.

This experiment was repeated upon clay taken from a brickyard in the suburbs of the city, with nearly the same result.

In order to calculate with some accuracy the value of this body of wealth, we cut out blocks of the clay, and found that on an average, a cubic foot, as it lies in the ground, weighs 120 pounds, as near as may be; making the specific gravity 1·92. The assay gives seven-tenths of a grain, say three cents' worth of gold to the cubic foot. Assuming the data already given, we get 4180 millions of cubic feet of clay under our streets and houses, in which securely lies 126 millions of dollars. And if, as is pretty certain, the corporate limits of the city would afford eight times this bulk of clay, we have more gold than has yet been brought, according to the statistics, from California and Australia.

It is also apparent that every time a cartload of clay is hauled out of a cellar, enough gold goes with it to pay for the carting. And if the bricks which front our houses could have brought to their surface, in the form of gold-leaf, the amount of gold which they contain, we should have the glittering show of two square inches on every brick.—*Am. Phil. Society Proceed.*, viii, 273.

4. *Notice of Chemical and Physical Apparatus, by Becker & Sons.*—The Sheffield [Yale] Scientific School has recently procured from Becker & Sons, 54 Columbia st., Brooklyn, N. Y., some instruments of such excellent device and workmanship that we deem it a duty to the scientific community to call attention to them. 1st, An air-pump for laboratory use. This has a single cylinder, the piston of which is solid and has a diameter of $3\frac{1}{4}$ inches and a seven inch stroke. The communication between the interior of the cylinder and the receiver is opened and closed by means of a two-way cock which is operated by the motion of the pump-handle. The instrument is compact, light, of simple construction but most accurate workanship, and from the absence of valves *is always ready* to furnish a good vacuum with a few minutes labor. It serves also as a condenser, and is adapted for transferring gases. It is the best instrument we have ever seen for the chemical laboratory. It cost $100.

2d. The Balances of Becker & Son, we are also prepared, after considerable experience in the use of those of Oertling, Deleuil and Hugershoff, and after having seen the instruments of nearly every balance-maker of this country and Europe, to pronounce the best known to us. They combine accuracy and delicacy with convenience and cheapness, to a higher degree than any Balances we have hitherto met with. There are now in the Analytical Laboratory here, three of the Becker Balances, viz., an assay balance, which, loaded with 5 grms. in either scale readily indicates $\frac{1}{50}$ of a milligramme, an Analytical Balance carrying 200 grms. in each pan readily indicating $\frac{1}{10}$th miligramme and a Physical Balance which with a load 5 kilogrammes is turned by one centigramme. See advertisement.

3d, A standard and mountain siphon Barometer with inch and millimeter graduations belonging to the Laboratory is a very beautiful and precise instrument.

The inch employed by Becker & Sons is derived directly from pendulum measurements of their own.

4th, finally, the Engineering Dept. has a Theodolite adapted also as a transit, of most perfect device and construction.

The self-recording meteorological instruments and beautiful aneroids which Becker & Sons have in operation at their *atelier*, are worthy the notice of all physicists. S. W. J.

Book Notices.—

1. *The Mathematical Works of Isaac Barrow, D.D., Master of Trinity College, Cambridge.* Edited for Trinity College by W. Whewell, D.D., Master of the College. Cambridge, 1860. 754 pages, 8vo.—This volume contains the lectures which Dr. Barrow delivered as Lucasian Professor of Mathematics at Cambridge; an office which he held from 1664 to 1670. These lectures consist of three series: the Lectiones Mathematicae, the Lectiones Opticae, and the Lectiones Geometricae; the first being on the general principles of mathematics, the second containing propositions of Optics proved geometrically, and the third treating of properties of Curve lines. The mathematical lectures are twenty-three in number, and were delivered in 1664, 5 and 6, but were not published till 1685.

After these, are given four lectures in which the author proposes to expound the method by which Archimedes invented his theorems concerning Cones and Spheres.

In the Lectiones Opticae, eighteen in number, Dr. Barrow treated of the theory of the foci of spherical surfaces and lenses; and explained the cause of the rainbow, simplifying Cartesius' calculations.

The Sectiones Geometricae, thirteen in number, first published in 1670, are full of curious methods of determining the areas and tangents of curves, many of which are very close anticipations of Newton's methods. The most noted of these is the method of drawing tangents to curves given in Lect. X, Art. 14. This method is justly held to be an anticipation of the Differential Calculus, and to approach very near to it.

Barrow's rule for finding the subtangent of a curve is this: "After constituting the equation to the curve, put $x-a$ and $y-e$ for the ordinates x and y; expand and reject all the terms in which there is no a or e; (for they destroy each other by the nature of the curve;) reject all the terms in which a or e are above the first power, or are multiplied together; (for they are of no value compared with the rest, as being infinitely small;) then put y for a and t the subtangent for e; and the subtangent is found." Barrow applies this method to determine the subtangents of several curves.

In 1669, Newton showed to Dr. Barrow some papers in which the method of fluxions was faintly indicated, and rules given for the rectification and quadrature of curves, and for drawing tangents to curves; and it appears that Newton had been in possession of this method from the year 1666.

In the year 1672, the celebrated Leibnitz communicated to some members of the Royal Society, certain researches relating to the differences of numbers, and in 1674 he announced that he possessed important theorems relative to the quadrature of the circle by series.

In 1676 Newton sent a letter to Oldenburg, which was to be shown to

Leibnitz, in which he describes the properties of his method of fluxions, as well for the determination of tangents, as the quadrature of curves. In 1677 Leibnitz sent to Oldenburg, to be communicated to Newton, a letter containing an account of his *Differential Calculus.* In 1684 Leibnitz published his new method in the Leipsic Acts; but Newton's method of Fluxions did not become generally known till the publication of the Principia in 1686. Leibnitz is admitted to have been the first to publish to the world the principles of the Differential Calculus; but Newton was unquestionably acquainted with the leading principles of the calculus before they were invented by Leibnitz; and Dr. Barrow had a method of drawing tangents to curves very similar to that furnished by the calculus, before either Newton or Leibnitz had devoted any special attention to the subject.

2. *Results of a scientific mission to India and High Asia undertaken between the years* 1854 *and* 1858, *by order of the court of Directors of the Honorable East India Company, by* HERMANN, ADOLPHE and ROBERT DE SCHLAGINTWEIT. With an atlas of panoramas, views and maps. Volume I. London: Trübner and Co. 1861. 512 pages, large quarto.—This volume contains the astronomical determinations of latitudes and longitudes and the magnetic observations made during the scientific journey of the brothers De Schlagintweit through India and High Asia. One of the chief objects of this journey was the completion of the magnetic survey of India, which had been commenced in 1846, by the late Captain Elliot in the Eastern Archipelago; but in consequence of the high interest evinced in it by the Directors of the East India company, the mission assumed a very general and extensive character. The brothers De Schlagintweit left England Sept. 20, 1854, and arrived in Bombay Oct. 26th. One of the brothers, Adolphe, was killed at Kashgar, in August, 1857;* the other two brothers arrived at Trieste, June 8, 1857. The results of this mission are to be embraced in nine quarto volumes of which the following are the titles:

I. Astronomical and magnetic observations.

II. Hypsometry, Barometrical and Trigonometrical observations.

III. Topical Geography, and Route Book of the Himalaya and Tibet.

IV and V. Meteorology and climate in general.

VI. Geology.

VII. Botany and Zoology, particularly with reference to geographical distribution.

VIII. Ethnography, comparative researches based on measurements, casts and photographs.

IX. Geographical aspects of India, the Himalaya, Tibet and Turkistan.

Volume I, recently received, contains a minute account of the methods of observation and calculation; with full details of the observations for the determination of geographical co-ordinates. The magnetic observations are very numerous, and are reported with great minuteness. These observations embraced the Declination of the magnetic needle; the horizontal intensity; the Dip and Vertical Intensity; and also the total intensity. The instruments employed were those generally adopted in the English magnetic surveys, and the magnetic intensity is expressed in the unit employed by General Sabine, and other English philosophers.

* See this Journal, vol. xxvi, 378, and xxix, 236.

The following is a summary of the magnetic observations made in the course of this Survey.

In column fourth S. L. signifies that the station was but slightly elevated above the level of the sea.

Results of the magnetic observations.

Station.	Latitude.	Longitude from Greenwich.	Height Eng. feet.	Declination East.	Dip North.	Total Intensity.
Suget,	36° 10′ N.	77° 50 ′E.	12,960	4° 21′·5	50° 12′·35	
Sumgal,	36 8	78 5	13,212		50 5·38	10·879
Tso Ka,	35 58	76 3	15,724		49 18·92	
Karakorum Pass,	35 46	77 30	18,341	3 33·6	49 13·75	10·933
Chorkonda,	35 33	75 56	13,790	2 53·4	48 43·25	
Skardo,	35 20	75 44	7,250	4 5·1	48 20·52	10·943
Tashing,	35 15	74 40	9,691	4 17·7	48 23·82	10·751
Sasser Pass,	35 6	77 27	17,753	3 31·9	48 17·68	
Daver,	34 34	74 46	7,718		47 41·65	
Kargil,	34 30	76 4	8,845	3 10·1	47 57·15	10·197
Dras,	34 28	75 43	9,951		46 51·45	10·122
Mozaferabad,	34 22	73 31	2,220	3 23·9	47 20·00	9·827
Leh,	34 8	77 14	11,527	3 22·6	46 52·64	10·113
Srinagger,	34 4	74 48	5,144	2 59·9	46 58·20	9·986
Peshaur,	34 3	71 33	1,250	2 27·9	46 25·75	10·889
Marri,	33 51	73 22	7,260	3 21·1	46 2·84	9·633
Tsomognalari,	33 39	78 38	14,010	3 21·8	46 34·05	9·972
Raulpindi,	33 36	72 59	1,674	3 5·5	45 55·71	9·904
Padum,	33 28	76 54	11,590	3 40·8	45 51·97	
Tsomoriri,	32 45	78 16	15,130	3 9·9	45 20·30	
Kardong,	32 33	77 0	10,233	3 23·3	44 28·44	10·960
Sultanpur,	31 57	77 5	3,830	3 2·6	43 52·19	
Mud,	31 55	78 1	12,421	3 43·5	44 17·83	
Dera Ismael Khan,	31 39	70 56	478	0 58·2	44 23·47	10·703
Vangtu Bridge,	31 37	77 54	4,210		43 22·80	
Lahor,	31 34	74 14	790	2 2·3	43 17·44	9·856
Rampur,	31 31	77 37	3,215		42 46·43	
Ussilla,	31 7	78 18	8,940		42 13·24	10·960
Simla,	31 6	77 7	7,091	2 55·5	42 30·00	9·709
Mana,	30 47	79 20	10,670	2 44·9	41 25·24	10·528
Milum,	30 34	79 54	11,640	2 40·3	40 31·91	10·489
Massuri,	30 28	77 59	7,549		41 15·12	10·807
Ambala,	30 21	76 48	1,026	2 26·2	40 48·40	
Nainital,	29 23	79 30	6,409	2 28·2	38 33·71	9·856
Shikarpur,	27 55	68 51	60		36 2·00	9·893
Aligarh,	27 53	78 3	760	1 37·3	36 58·90	
Kathmandu,	27 42	85 12	4,350	2 35·8	37 34·24	8·734
Dibrugarh,	27 32	94 57	395	0 46·4	38 30·35	9·882
Falut,	27 6	87 59	12,042	2 24·8	36 54·96	8·816
Darjiling,	27 3	88 15	7,168	2 48·0	36 32·97	8·152
Tonglo,	27 1	88 3	10,080	2 30·5	36 25·04	8·539
Narigun,	26 53	92 6	3,615	4 43·0	37 8·11	9·114
Lakhnau,	26 51	80 55	520	2 37·4	35 18·55	10·019
Sigauli.	26 46	84 44	260		35 40·10	
Udelguri,	26 45	91 56	352	2 36·3	36 27·65	9·624
Tezpur,	26 34	92 46	239	0 22·5	37 14·93	9·746
Kissenganj,	26 6	87 56	140	2 20·2	35 11·95	8·187
Gohatti,	26 5	91 43	134	2 0·1	35 19·15	9·541
Patna,	25 37	85 7	170	1 53·9	33 32·96	9·215
Benares,	25 18	82 59	325	1 50·3	32 41·25	9·294
Cherra Punji,	25 14	91 40	4,164	2 20·4	33 .7·27	9·449
Surajganj,	24 22	89 43	S. L.		32 3·50	

TABLE—(continued.)

Station.	Latitude.	Longitude from Greenwich.	Height Eng. feet.	Declination East	Dip North.	Total Intensity.
Rampur Bolea,	24° 21′ N.	88° 34′ E.	54		32° 0′·77	7·904
Sager,	23 50	78 43	1,880		29 58·84	
Dhaka,	23 42	90 20	s. l.	2° 21′·2	31 1·23	
Bhuj,	23 17	69 40	283	0 12·0	28 25·00	9·109
Jablpur,	23 9	79 56	1,480	1 10·5	28 31·14	9·863
Kulna,	22 45	89 36	s. l.	2 30·4	29 19·85	
Calcutta,	22 33	88 20	s. l.	2 25·1	28 14·84	9·113
Nagri,	20 25	78 52	850		22 49·99	9·367
Bombay,	18 53	72 49	s. l.	0 19·1	19 6·60	8·475
Puna,	18 30	73 52	1,819		19 2·25	
Mahabaleshvar,	17 55	73 38	4,396		16 25·50	
Rajamandri,	17 10	81 46	s. l.	1 24·8	16 23·53	9·197
Kaladghi,	16 12	75 29	1,720	0 30·0	14 27·25	
Bellari,	15 8	76 53	1,580	0 21·0	11 59·68	8·834
Madras,	13 4	80 13	s. l.	0 59·3	7 52·34	8·100
Utakamand,	11 23	76 43	7,278	0 57.0	4 27·32	8·862
Utatur,	11 4	78 51	280		2 50·08	
Galle,	6 2	80 10	s. l.	0 41·0	-7 40·90	8·076

3. *Microscopic Anatomy of the Lumbar Enlargement of the Spinal Cord;* by JOHN DEAN, M.D. (Communicated to the American Academy of Arts and Sciences by Prof. Jeffries Wyman, Nov. 14, 1860.)—This is a paper of great value, displaying extensive and patient research in a very important but little cultivated department of anatomy. This indefatigable investigator of the nervous system has confirmed the discoveries of J. L. Clarke, published in the Transactions of the Royal Society, 1859, and added other important discoveries of his own.

Our limits do not permit us to give even a synopsis of this valuable paper but we cite the following account of the author's method of preparing sections of the spinal cord for microscopic examination:

"The method of preparation usually employed was a modification of Gerlach's and Clarke's although many others were employed, according to the object in view. The following method gave the best results from which to make drawings. Thin sections from the cord, hardened in alcohol, were washed a few minutes in pure water, and then immersed in glycerine, to which Gerlach's solution of carmine (solution of carmine in water to which a few drops of strong ammonia have been added) previously filtered, had been added; in this the sections were allowed to remain from four to eight hours according to the tint desired (a light tint interfering least with the details and sharpness of outline). I have been able to obtain more delicately colored specimens and more clearly defined structure by the use of glycerine than by any other method. The sections are then washed first in pure water, afterwards with strong alcohol, in which they are allowed to remain about an hour, and are now ready for preparation with turpentine according to Clarke's method; [*i. e.*, to lay them on a slide and drop on turpentine from time to time until the alcohol is replaced by turpentine] they may be put in Canada Balsam, or, as I have found very advantageous, in thick, colorless copal varnish, which often preserves minute details better than balsam. Although Stilling and others have found much fault with Clarke's method of preparation, on account of the too great transparency it sometimes gives, I am convinced

that, with practice and some slight modifications, it is the only one suited to the minute study of the cord, other methods seeming to me, after thorough trial, quite unsatisfactory as compared with Clarke's. As a hardening material, I have often employed chromic acid with considerable advantage; but when coloring matter is used, alcohol is most suitable, and is certainly much easier to succeed with."

4. *Summary of Medical Science;* edited by WALTER S. WELLS, M.D. 304 pages. 8vo. New York, Charles T. Evans.—This is an epitome of Medical Literature selected from European and American Medical Journals, including also original articles, to be issued semi-annually. The arrangement and design of this work will make it a valuable repertory of the current views of the medical profession, especially to those who are not supplied with a very large variety of medical periodicals.

5. *Report on the Geology and Agriculture of the State of Mississippi;* by EUGENE W. HILGARD, Ph.D., State Geologist. Jackson, Miss., 1860. 8vo, pp. 391, accompanied by a Geological Map and Sections.—This Report, announced in our last, is a valuable contribution to our knowledge of the Cretaceous and Tertiary strata which cover by far the larger portion of the surface of Mississippi. We had marked for extract the remarks of Dr. Hilgard on the general relations of these two great groups of strata, as unfolded in his Report, but want of space compels us to postpone their citation now. The Geological Map of Mississippi which accompanies the Report is a valuable addition to our knowledge of the exact tendencies of the several formations in this State, made more valuable by the profiles of the Mississippi formations which are given. We hope to notice some points (both Geological and Agricultural) in this report again.

6. *First Biennial Report of the progress of the Geological Survey of Michigan*, embracing observations on the Geology, Zoology and Botany of the Lower Peninsula, made to the Governor, Dec. 31, 1860. Lansing, 1861. 8vo.—A fragment only of the complete Report (270 pages) has reached us. It is a satisfaction to see the work resumed in Michigan which Houghton so well began many years ago. In glancing at the Chapter on Peat, which occurs abundantly in Michigan, we see no allusion to the valuable researches of Prof. Johnson made for the Connecticut State Agricultural Society, the agricultural value of which is such as to entitle them to special notice. When the complete Report reaches us we shall resume our notice.

BROCHURES.

ZOOLOGICAL AND ANATOMICAL.—

On the Rhizopodal Fauna of the Mediterranean, compared with that of the Italian and other Tertiary deposits. By T. RUPERT JONES, Esq., F.G.S., and W. K. PARKER, Esq., Mem. M.S. 1860. 8vo, pp. 16.

Synopsis of American Cretaceous Brachiopoda. Proc. Acad. Nat. Sci., Jan. 1861. By W. M. GABB.

On the genus Raphidophora, Serville; with descriptions of four species from the Caves of Kentucky, and from the Pacific Coast. Bost. Soc. Nat. Hist. By SAMUEL H. SCUDDER.

Synonymy of the Cyclades, a family of Acephalous Mollusca. Part 2. By TEMPLE PRIME. Proc. Acad. Nat. Sci. Phil., July, 1861.

Croonian Lecture.—On the Arrangement of the Muscular Fibres of the Ventricular Portion of the Heart of the Mammal. By JAMES PETTIGREW, Esq.

GEOLOGICAL.—

Documents anciens et nouveaux sur la faune primordiale et le Système Taconique en Amérique. Par M. J. BARRANDE. With two plates.

Notes on the Cretaceous and Carboniferous Rocks of Texas. By JULES MARCOU. Boston, 1861.

On the Occurrence of Flint-implements, associated with the Remains of Animals of extinct species in beds of a late geological period, in France at Amiens and Abbeville, and in England at Hoxne. By JOSEPH PRESTWICH, F.R.S., F.G.S., etc. London, 1861. 4to, pp. 40.

On the supposed identity of the Paradoxides Harlani, Green, with the Paradoxides spinosus, Boeck. By ALBERT ORDWAY.

On the Fossils brought from the Arctic Regions in 1859, by Captain Sir F. L. M'Clintock. By the Rev. SAMUEL HAUGHTON, M.A., etc.

On Cyclostigma, a new genus of fossil plants from the Old Red Sandstone of Kiltorcan, Co. Kilkenny; and on the General Law of Phyllotaxis in the Natural Orders, —Lycopodiaceæ, Equisetaceæ, Filices, etc. By the Rev. SAMUEL HAUGHTON, etc.

On the Discovery of some Fossil Remains near Bahia in South America. By S. ALLPORT, Esq.

On the Structure of the Northwestern Highlands, and the relations of the Gneiss, Red Sandstone, and Quartzite of Sutherland and Ross-shire. By JAMES NICOL, F.G.S., F.R.S.E., etc.

Address delivered at the Anniversary Meeting of the Geological Society of London, on the 15th of February, 1861; prefaced by the announcement of the award of the Wollaston Medal and proceeds of the Donation-fund for the same year. By LEONARD HORNER, Esq., F.R.S. L. & E., President of the Society. London, 1861.

MISCELLANEOUS.—

Report made to the Navy Department by the Board of U. S. Naval Engineers, to determine the relative economy of using steam with different degrees of expansion. Washington, 1861. 8vo, pp. 38.

Publications of Isaac Lea on Recent Conchology. Bibliography. January 1, 1861.

Report of the Trustees of the Museum of Comparative Zoology, 1861. Boston, 1861.

The Second Annual Report of the Trustees of the Cooper Union for the Advancement of Science and Art. January 1, 1861. New York, 1861.

New York State Agricultural College, Ovid, Seneca Co., N. Y.—Charter, Ordinances, Regulations and Course of Studies. 1861. Albany, 1861. 8vo, pp. 16.

The Theory and Art of Bread-making. A new process without the use of Ferment. By E. N. HORSFORD, Rumford Professor in Harvard University, Cambridge. 1861.

Catalogue of Telegraph Material, manufactured by Charles T. & J. N. Chester, 104 Centre Street, New York. With Appendix. New York.

Appendix to Messrs. Stevenson's Answer to Sir David Brewster's Reply regarding Dioptric Lights. By D. & T. STEVENSON, Civil Engineers. Edinburgh, 1860.

Reply to Professor Tyndall's Remarks, in his work "on the Glaciers of the Alps," relating to Rendu's "Théorie des Glaciers." By JAMES DAVID FORBES, D.C.L., LL.D., F.R.S., etc. Edinburgh, 1860.

Voyage d'André Michaux en Canada. Depuis le Lac Champlain jusqu'a la Baie d'Hudson. Par O. BRUNET. Quebec: Bureau de l'Abeille. 1861.

PERSONAL.

Correction.—In our notice of the death of Dr. Robb, p. 150, his name was incorrectly given as CHARLES—it was JAMES; and his labors in his chair commenced in 1837, not as stated in 1827.

LORING W. BAILEY, Esq., late of Harvard College—has succeeded to the chair made vacant by the death of Dr. Robb in the University of New Brunswick, at Fredrickton, N. B., British N. A.

The numerous friends of the late Prof. J. W. Bailey of West Point Military Academy, whose memory is so warmly cherished by all who knew him, will rejoice to welcome to the active walks of science a son who in kindred branches of investigation has already shown marked ability, and to know that he has already achieved so honorable a post.

P. S. *Sept. 2d.*—The telegraphic report of an earthquake at Louisville, Cincinnati and other central parts of the United States, reaches us at this moment. Will correspondents in the affected district send us their observations?